ACTA·COLL·MED·PHILAD·INSTIT·A·D·MDCCLXXXVII

NON SIBI SED TOTI

TRANSACTIONS

OF THE

COLLEGE OF PHYSICIANS

OF PHILADELPHIA.

VOLUME THE FIFTEENTH.

TRANSACTIONS

OF THE

COLLEGE OF PHYSICIANS

OF

PHILADELPHIA.

THIRD SERIES.

VOLUME THE EIGHTH.

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COLLEGE OF PHYSICIANS OF PHILADELPHIA.

1886.

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LIST

OF THE

PRESIDENTS OF THE COLLEGE FROM THE TIME OF ITS INSTITUTION.

ELECTED	
1787.	JOHN REDMAN, M.D.
1805.	WILLIAM SHIPPEN, M.D.
1809.	ADAM KUHN, M.D.
1818.	THOMAS PARKE, M.D.
1835.	THOMAS C. JAMES, M.D.*
1835.	THOMAS T. HEWSON, M.D.
1848.	GEORGE B. WOOD, M.D., LL.D.
1879.	W. S. W. RUSCHENBERGER, M.D.
1883.	ALFRED STILLÉ, M.D., LL.D.
1884.	SAMUEL LEWIS, M.D.†
1884.	J. M. DA COSTA, M.D., LL.D.
1886.	S. WEIR MITCHELL, M.D.

* Died four months after his election.

† Resigned on account of ill health.

FELLOWS

OF THE

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

JUNE, 1886.

[Non-Resident Fellows are marked thus (*).]

ELECTED

1883. ABBOT, GRIFFITH E., Ph. D., M.D.

1870. ADLER, JOHN M., M.D.

1859. AGNEW, D. HAYES, M.D., LL.D., Professor of Surgery in the University of Pennsylvania, Consulting Surgeon to the Orthopædic Hospital and to St. Christopher's Hospital.

*1876. ALISON, ROBERT H., M.D.

1867. ALLEN, HARRISON, M.D., Emeritus Professor of Physiology in the University of Pennsylvania, Consulting Surgeon to the Mission Hospital.

1873. ALLIS, OSCAR H., M.D., Surgeon to the Presbyterian Hospital and to the Howard Hospital.

1869. ANDREWS, T. HOLLINGSWORTH, M.D., Surgeon to the Howard Hospital, Consulting Surgeon to the Hospital of the Good Shepherd, Radnor.

*1882. ASHBRIDGE, RICHARD, M.D., Assistant Surgeon U. S. Navy.

1863. ASHHURST, JOHN, JR., M.D., Professor of Clinical Surgery in the University of Pennsylvania, Surgeon to the Children's Hospital, Consulting Surgeon to St. Christopher's Hospital and to the Hospital of the Good Shepherd, Radnor.

ELECTED

1865. Ashhurst, Samuel, M.D., Surgeon to the Children's Hospital.

1835. Ashmead, William, M.D.

1857. Atlee, Walter F., M.D., Consulting Physician and Surgeon to St. Luke's Hospital, Bethlehem.

1852. Bache, Thomas Hewson, M.D.

1883. Baer, Benjamin F., M.D., Professor of Obstetrics and Gynecology in the Philadelphia Polyclinic and College for Graduates in Medicine, Obstetrician to the Maternity Hospital.

1879. Baker, Washington H., M.D., Obstetrician to the State Hospital for Women and Infants, Assistant Physician to the Philadelphia Lying-in Charity and to St. Christopher's Hospital, Assistant Surgeon to the Orthopædic Hospital.

1876. Baldwin, Louis K., M.D., Examining Physician to the Hospital of the Good Shepherd, Radnor.

1880. Bartholow, Roberts, M.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College.

1883. Baum, Charles, M.D.

1873. Baxter, H. F., M.D.

1883. Beates, Henry, M.D.

1860. Benner, Henry D., M.D.

1874. Bennett, W. H., M.D., Physician to the Episcopal Hospital and to St. Christopher's Hospital.

1884. Biddle, Alexander W., M.D.

1884. Biddle, Thomas, M.D.

*1866. Black, J. J., M.D.

*1867. Boardman, Charles H., M.D.

1859. Boker, Charles S., M.D., Surgeon to St. Joseph's Hospital.

1884. Bradford, Thomas Hewson, M.D.

1856. Brinton, John H., M.D., Professor of the Practice of Surgery and Clinical Surgery in the Jefferson Medical College, Surgeon to St. Joseph's Hospital, and Consulting Surgeon to the Southwestern Hospital of Philadelphia.

ELECTED

1878. Bruen, Edward T., M.D., Assistant Professor of Physical Diagnosis in the University of Pennsylvania, Physician to the Philadelphia Hospital, and Examining Physician to the Hospital of the Good Shepherd, Radnor.

*1851. Bullock, William R., M.D.

1870. Burnett, C. H., M.D., Professor for Diseases of the Ear in the Philadelphia Polyclinic, Aurist to the Presbyterian Hospital, Consulting Aurist to Pennsylvania Institution for the Deaf and Dumb.

1886. Cadwalader, Charles E., M.D.

1885. Chapin, John B., M.D., Physician to the Pennsylvania Hospital for the Insane.

1880. Chapman, Henry C., M.D., Professor of the Institutes of Medicine and of Medical Jurisprudence in the Jefferson Medical College.

1868. Cheston, D. Murray, M.D., Physician to the Children's Hospital.

1873. Clark, Leonardo S., M.D.

1872. Cleemann, Richard A., M.D.

*1842. Clymer, Meredith, M.D.

1871. Cohen, J. Solis, M.D., Professor of Diseases of the Throat and Chest in the Philadelphia Polyclinic and College for Graduates in Medicine, and Physician to the German Hospital.

1866. Cruice, R. B., M.D., Surgeon to St. Joseph's Hospital.

1873. Cruice, W. R., M.D.

1884. Curtin, R. G., M.D., Lecturer on Physical Diagnosis at the University of Pennsylvania, Assistant Physician to the University Hospital and the Philadelphia Hospital.

1884. Da Costa, J. C., M.D., Gynecologist to the Jefferson Medical College Hospital.

ELECTED

1858. Da Costa, J. M., M.D. Professor of the Principles and Practice of Medicine in the Jefferson Medical College, Physician to the Pennsylvania Hospital, Consulting Physician to the Children's Hospital and to the Northern Dispensary.

1859. Darrach, James, M.D., Consulting Surgeon to the Germantown Hospital.

1874. Deakyne, A. C., M.D.

1870. Deal, L. J., M.D.

1885. Dercum, Francis J., M.D.

1864. Downs, R. N., M.D., Consulting Physician to the Germantown Hospital.

1884. Drysdale, T. M., M.D.

1864. Duer, E. L., M.D., Accoucheur to the Philadelphia Hospital, Surgeon to the State Hospital for Women and Infants, Visiting Physician to the Preston Retreat.

1871. Duhring, L. A., M.D., Clinical Professor of Skin Diseases in the Hospital of the University of Pennsylvania, Dermatologist to the Philadelphia Hospital, and Consulting Physician to the Philadelphia Dispensary for Skin Diseases.

1881. Dulles, Charles Winslow, M.D., Surgeon to the Outpatient Department of the Hospital of the University of Pennsylvania and of the Presbyterian Hospital.

1863. Dunglison, Richard J., M.D.

*1871. Dunglison, Thomas R., M.D.

*1849. Dunott, Justus, M.D.

1860. Dunton, William R., M.D., Consulting Physician to the Germantown Hospital.

1882. Edwards, Joseph F., M.D.

*1880. Eskridge, J. T., M.D.

1868. Evans, Horace Y., M.D., Physician to the Charity Hospital.

ELECTED

1884. Fenton, Thomas H., M.D.

1872. Finn, W. H., M.D., Surgeon to St. Christopher's Hospital.

1866. Fischer, Emil, M.D.

1884. Fisher, Henry M., M.D.

1862. Forbes, William S., M.D., Professor of Anatomy in the Jefferson Medical College, Surgeon to the Episcopal Hospital.

1870. Ford, W. H., M.D.

1884. Formad, H. F., M.D.

1885. Fox, Joseph M., M.D.

1864. Fricke, Albert, M.D.

1870. Gardette, E. B., M.D.

1873. Gerhard, George S., M.D.

1864. Getchell, F. H., M.D., Obstetric Physician to the Catherine Street Dispensary.

1885. Girvin, Robert M., M.D.

*1848. Given, Robert A., M.D.

1884. Godey, Harry, M.D.

1868. Goodell, William, M.D., Professor of Clinical Gynecology in the University of Pennsylvania, Physician in Charge of the Preston Retreat, Consulting Physician to the Lying-in Department of the Northern Dispensary.

1867. Goodman, H. Earnest, M.D., Surgeon to Wills Hospital and to the Orthopædic Hospital, Consulting Surgeon to the State Hospital for Women and Infants.

1885. Graham, John, M.D.

1864. Granger, William H., M.D.

1870. Grier, M. J., M.D.

1883. Griffith, J. P. Crozer, M.D.

1842. Griscom, John D., M.D.

1883. Gross, Ferdinand H., M.D., Surgeon to the German Hospital.

ELECTED

1868. Gross, Samuel W., M.D., LL.D., Professor of the Principles of Surgery and Clinical Surgery in the Jefferson Medical College.

1871. Grove, John H., M.D., Surgeon to St. Mary's Hospital.

1863. Hall, A. Douglass, M.D., Surgeon to Wills Hospital.

*1859. Hammond, William A., M.D.

1886. Hansell, Howard F., M.D., Adjunct Professor of Diseases of the Eye in the Philadelphia Polyclinic, and Ophthalmic and Aural Surgeon to the Southwestern Hospital.

1865. Harlan, George C., M.D., Surgeon to Wills Hospital, Ophthalmic and Aural Surgeon to the Pennsylvania Hospital.

1863. Harlow, Lewis D., M.D.

1862. Harris, Robert P., M.D.

1885. Harte, Richard H., M.D.

1851. Hartshorne, Henry, M.D., LL.D.

*1849. Hastings, John, M.D.

1855. Hatfield, Nathan L., M.D., Consulting Physician to the Northern Dispensary.

1872. Hays, I. Minis, M.D.

1882. Hearn, Joseph, M.D., Surgeon to the Hospital of the Jefferson Medical College, and to the Philadelphia Hospital.

1884. Henry, Frederick P., M.D., Professor of Clinical Medicine in the Philadelphia Polyclinic, Physician to the Episcopal Hospital.

1853. Hewson, Addinell, M.D.

1872. Hinkle, A. G. B., M.D.

1885. Holland, James W., M.D., Professor of Chemistry in the Jefferson Medical College.

1879. Hopkins, William Barton, M.D., Surgeon to the Episcopal Hospital, and to the Outpatient Department of the Pennsylvania Hospital.

ELECTED

1867. Horn, George H., M.D.

1884. Horwitz, Phineas T., M.D.

1868. Howell, Samuel B., M.D., Professor of Mineralogy and Geology in the University of Pennsylvania.

*1882. Hughes, Daniel E., M.D.

1881. Huidekoper, Rush Shippen, M.D., Professor of Internal Pathology and Contagious Diseases in the Veterinary Faculty of the University of Pennsylvania.

1884. Hunt, J. Gibbons, M.D.

1854. Hunt, William, M.D., Surgeon to the Pennsylvania Hospital and to the Orthopædic Hospital.

1863. Hutchinson, James H., M.D., Physician to the Pennsylvania Hospital and to the Children's Hospital.

1871. Ingham, James V., M.D.

1885. Jackson, Edward, M.D.

*1864. Jones, S. P., M.D.

1885. Judd, Leonardo Da Vinci, M.D.

1867. Judson, Oliver A., M.D.

1886. Jurist, Louis, M.D.

1877. Keating, John M., M.D., Obstetrician to the Philadelphia Hospital, Physician to St. Joseph's Hospital.

1849. Keating, William V., M.D., Physician to St. Joseph's Hospital.

1867. Keen, William W., M.D., Professor of the Principles and Practice of Surgery in the Woman's Medical College of Pennsylvania, Surgeon to St. Mary's Hospital, Consulting Surgeon to the Mission Hospital, and to the Philadelphia Home for Incurables.

1852. Kennedy, Alfred L., M.D.

ELECTED

*1844. King, Charles R., M.D.

1875. Kirkbride, J. J., M.D., Physician for Outpatients to the Pennsylvania Hospital.

*1865. LaRoche, C. Percy, M.D.

1883. Leffmann, Henry, M.D., Professor of Chemistry and Metallurgy in the Pennsylvania College of Dental Surgery and in the Wagner Free Institute of Science.

1851. Leidy, Joseph, M.D., LL.D., Professor of Anatomy in the University of Pennsylvania.

1885. Leidy, Philip, M.D.

1855. Lewis, Francis W., M.D.

1877. Lewis, Morris J., M.D., Physician to the Episcopal Hospital, Assistant Physician to the Children's Hospital, and to the Orthopædic Hospital and Infirmary for Nervous Diseases.

1849. Lewis, Samuel, M.D.

1877. Longstreth, Morris, M.D., Lecturer on Pathological Anatomy in the Jefferson Medical College, Physician, and Pathologist and Curator to the Pennsylvania Hospital.

1849. Ludlow, John L., M.D., Physician to the Philadelphia Hospital and to the Presbyterian Hospital.

1875. McClellan, George, M.D., Surgeon to the Philadelphia Hospital.

1871. McFerran, J. A., M.D., Physician to the Gynecological Hospital and Infirmary for Diseases of Children.

*1885. Mallet, John W., M.D.

*1850. Mayer, Edward H., M.D.

1885. Mays, Thomas J., M.D.

ELECTED

1868. Mears, J. Ewing, M.D., Professor of Anatomy and Clinical Surgery in the Pennsylvania College of Dental Surgery, Demonstrator of Surgery in the Jefferson Medical College and Gynecologist to the Hospital of the same, Surgeon to St. Mary's Hospital.

1875. Meigs, Arthur V., M.D., Physician to the Pennsylvania Hospital and to the Children's Hospital.

1884. Mifflin, Houston, M.D.

1881. Mills, Charles K., M.D., Professor of Diseases of the Mind and Nervous System in the Philadelphia Polyclinic and College for Graduates in Medicine, Lecturer on Mental Diseases in the University of Pennsylvania, Neurologist to the Philadelphia Hospital, and Consulting Physician to the Insane Department of the Philadelphia Hospital.

1856. Mitchell, S. Weir, M.D., Physician to the Orthopædic Hospital and Infirmary for Nervous Diseases, Consulting Physician to the State Hospital for Women and Infants.

1882. Montgomery, Edward E., M.D., Professor of Gynecology and Clinical Gynecology in the Medico-Chirurgical College, and Obstetrician to the Philadelphia Hospital.

1863. Morehouse, George R., M.D., Physician to St. Joseph's Hospital.

1886. Morris, Caspar, M.D.

1883. Morris, Henry, M. D.

1856. Morris, J. Cheston, M.D., Consulting Physician to the Mission Hospital.

1861. Morton, Thomas G., M.D., Surgeon to the Pennsylvania Hospital and to the Orthopædic Hospital, Consulting Surgeon to the Jewish Hospital, Emeritus Surgeon to Wills Hospital.

1864. Moss, William, M.D.

1882. Musser, John H., M.D., Chief of the Medical Dispensary of the Hospital of the University of Pennsylvania, Pathologist to the Presbyterian Hospital, and Physician to the Philadelphia Hospital.

1883. Musser, Milton B., M.D.

ELECTED

1886. Neff, Joseph F., M.D., Attending Physician to the Jefferson Medical Hospital and the Philadelphia Hospital, Lecturer on Urinary Pathology at the Jefferson Medical College, Physician to Outpatient Department of Pennsylvania Hospital.

1869. Norris, Herbert, M.D., Physician to the Catharine Street Dispensary.

1865. Norris, Isaac, Jr., M.D.

1866. Norris, William F., M.D., Clinical Professor of Diseases of the Eye in the Hospital of the University of Pennsylvania, Surgeon to Wills Hospital.

1884. Oliver, Charles A., M.D., Ophthalmic and Aural Surgeon to St. Mary's Hospital and to the Maternity Hospital, Visiting Physician and Ophthalmologist to the State Hospital for the Insane, Norristown.

1884. O'Neill, J. W., M.D.

1885. Osler, William, M.D., Professor of Clinical Medicine in the University of Pennsylvania.

1858. Packard, John H., M.D., Surgeon to the Pennsylvania Hospital and to St. Joseph's Hospital.

1864. Pancoast, William H., M.D., Professor of Anatomy in the Medico-Chirurgical College, Consulting Surgeon to the Charity Hospital and to the Pennsylvania Free Dispensary for Skin Diseases.

1882. Parish, William H., M.D., Professor of Anatomy in the Woman's Medical College, and Obstetrician to the Philadelphia Hospital.

*1854. Parrish, Joseph, M.D.

1883. Parvin, Theophilus, M.D., Professor of Obstetrics and Diseases of Women and Children in the Jefferson Medical College.

1854. Penrose, R. A. F., M.D., LL.D., Professor of Obstetrics and Diseases of Women and Children in the University of Pennsylvania, Consulting Obstetrician to the State Hospital for Women and Infants, Visiting Physician to the Preston Retreat.

ELECTED

1868. Pepper, William, M.D., LL.D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania.

1884. Perkins, Francis M., M.D., Ophthalmic and Aural Surgeon to the Dispensary Department of St. Mary's Hospital, and Visiting Ophthalmic Surgeon to the Hospital of the Good Shepherd at Radnor.

1883. Piersol, George A., M.D., Assistant Demonstrator of Normal Histology in the University of Pennsylvania.

1872. Porter, William G., M.D., Surgeon to the Presbyterian Hospital and to the Philadelphia Hospital.

1885. Potter, Thomas C., M.D.

1883. Randolph, N. Archer, M.D., Assistant Demonstrator of Physiology in the University of Pennsylvania.

1866. Reed, Thomas B., M.D., Surgeon to the Presbyterian Hospital.

1842. Reese, John J., M.D., Professor of Medical Jurisprudence in the University of Pennsylvania, Physician to St. Joseph's Hospital, and to the Gynecological Hospital and Infirmary for Diseases of Children.

1885. Reichert, Edward T., M.D., Professor of Physiology in the University of Pennsylvania.

1884. Rex, Francis M., M.D.

1883. Rex, Oliver P., M.D., Physician to the Jefferson Medical College Hospital.

1871. Richardson, Elliot, M.D., Lecturer on Practical Obstetrics in the University of Pennsylvania, Demonstrator of Obstetrics in the same, Obstetrician to the Philadelphia Hospital and Gynecologist to the Pennsylvania Hospital.

1869. Richardson, Joseph G., M.D., Professor of Hygiene and Demonstrator of Histology in the University of Pennsylvania, and Physician for Outpatients and Microscopist to the Pennsylvania Hospital.

*1857. Richardson, Tobias G., M.D.

ELECTED

1882. Roberts, A. Sydney, M.D., Visiting Surgeon to the Philadelphia Hospital and to the Orthopædic Dispensary of the University Hospital, Instructor in Orthopædic Surgery in the University of Pennsylvania.

1878. Roberts, John B., M.D., Professor of Applied Anatomy and Operative Surgery in the Philadelphia Polyclinic, Surgeon to St. Mary's Hospital.

1843. Rodman, Lewis, M.D., Visiting Physician to the Preston Retreat.

1838. Ruschenberger, W. S. W., M.D., Medical Director U. S. N., retired.

*1852. Sargent, Fitz William, M.D.

*1864. Sargent, Winthrop, M.D.

1866. Schaffer, Charles, M.D.

1870. Schell, Henry S., M.D., Surgeon to Wills Hospital, Ophthalmic and Aural Surgeon to the Children's Hospital.

1875. Seyfert, Theodore H., M.D., Physician to the Gynecological Hospital and Infirmary for Diseases of Children.

1884. Shaffner, Charles, M.D.

1877. Shakespeare, Edward O., M.D., Lecturer on Refraction and Accommodation of the Eye, and on Operative Ophthalmic Surgery, in the University of Pennsylvania, Pathologist and Ophthalmologist to the Philadelphia Hospital.

1868. Shapleigh, E. B., M.D.

1876. Shippen, Edward, M.D., U. S. N.

1880. Simes, J. H. C., M.D., Professor of Genito-Urinary and Venereal Diseases in the Philadelphia Polyclinic and College for Graduates in Medicine, and Assistant Surgeon to the Episcopal Hospital.

1873. Simpson, James, M.D., Physician to St. Mary's Hospital.

1872. Sinkler, Wharton, M.D., Physician to the Orthopædic Hospital and Infirmary for Nervous Diseases.

*1863. Smith, A. K., M.D., U. S. A.

ELECTED

*1864. Smith, Edward A., M.D.

*1856. Smith, R. K., M.D.

1884. Smith, Robert Meade, M.D.

1864. Spooner, Edward A., M.D.

1875. Starr, Louis, M.D., Physician to the Children's Hospital, Clinical Professor of Diseases of Children in the University of Pennsylvania.

1884. Stelwagon, H. W., M.D., Chief of the Skin Dispensary of the University of Pennsylvania, Physician to the Philadelphia Dispensary for Skin Diseases, Dermatologist to the Northern Dispensary and to the Howard Hospital, Lecturer on Dermatology in the Woman's Medical College.

1842. Stillé, Alfred, M.D., LL.D., Professor Emeritus of the Theory and Practice of Medicine in the University of Pennsylvania, Consulting Physician to the State Hospital for Women and Infants.

1846. Stocker, Anthony E., M.D.

1871. Strawbridge, George, M.D., Clinical Professor of Diseases of the Ear in the Hospital of the University of Pennsylvania, Surgeon to Wills Hospital, Ophthalmic Surgeon to the Presbyterian Hospital, Surgeon to the Eye and Ear Department of the Philadelphia Dispensary.

1884. Stryker, S. S., M.D., Obstetrician to the Philadelphia Hospital.

1886. Taylor, John Madison, M.D.

1867. Taylor, R. R., M.D.

1867. Thomas, Charles H., M.D., Surgeon to the Philadelphia Hospital.

1869. Thomson, William, M.D., Honorary Professor of Ophthalmology in the Jefferson Medical College, and Ophthalmic Surgeon to the Hospital of the same.

*1854. Tilden, W. P., M.D.

*1870. Turner, A. Paul, M.D.

ELECTED

1866. Tyson, James, M.D., Professor of General Pathology and Morbid Anatomy in the University of Pennsylvania, Physician to the Philadelphia Hospital.

*1852. Tyson, James L., M.D.

1864. Vandyke, E. B., M.D.

1873. Van Harlingen, Arthur, M.D., Professor of Diseases of the Skin in the Philadelphia Polyclinic and College for Graduates in Medicine, Consulting Physician to the Dispensary for Skin Diseases, Dermatologist to the Howard Hospital.

1883. Vinton, Charles Harrod, M.D.

1885. Walker, James B., M.D.

1886. Watson, E. W., M.D.

1875. Webb, William H., M.D.

1883. Welch, William M., M.D., Physician to Municipal Hospital for Contagious Diseases.

1884. Wharton, H. R., M.D., Surgeon to the Children's Hospital, Instructor in Clinical Surgery in the University of Pennsylvania and Assistant Surgeon to the Hospital of the same, and Attending Physician to the Pennsylvania Institution for the Deaf and Dumb.

1883. Whelen, Alfred, M.D.

1878. White, J. William, M.D., Demonstrator of Surgery and Lecturer on Venereal Diseases in the University of Pennsylvania, Surgeon to the Philadelphia Hospital.

1880. Willard, DeForest, M.D., Lecturer on Orthopædic Surgery in the University of Pennsylvania, Surgeon to the Presbyterian Hospital, Consulting Surgeon to Home for Crippled Children.

*1878. Williamson, Jesse, M.D.

1851. Wilson, Ellwood, M.D., Consulting Physician to the Philadelphia Lying-in Charity, Visiting Physician to the Preston Retreat.

ELECTED

1881. Wilson, H. Augustus, M.D.

1874. Wilson, J. C., M.D., Lecturer on Physical Diagnosis in the Jefferson Medical College and Physician to the Hospital of the same, Physician to the Philadelphia Hospital.

1884. Wirgman, Charles, M.D.

1871. Wistar, Thomas, M.D.

1848. Wister, Caspar, M.D., Consulting Physician to the Philadelphia Dispensary.

1852. Wister, Owen Jones, M.D., Consulting Surgeon to the Germantown Hospital.

1865. Wood, Horatio C., M.D., Professor of Materia Medica, Pharmacy, and General Therapeutics in the University of Pennsylvania and Clinical Professor of Diseases of the Nervous System in the Hospital of the same.

1880. Woodbury, Frank, M.D., Professor of Materia Medica and Therapeutics in the Medico-Chirurgical College of Philadelphia, Physician to the German Hospital.

1866. Woods, D. F., M.D., Physician to the Presbyterian Hospital.

1878. Wormley, Theodore G., M.D., LL.D., Professor of Chemistry in the University of Pennsylvania.

1860. Wurts, Charles Stewart, M.D.

1861. Yarrow, Thomas J., M.D.

*1840. Zanzinger, William S., M.D.

[It is particularly requested that any change of appointment, etc., may be communicated to the Committee of Publication before the first of July, in each year, in order that the above list may be made as correct as possible.]

ASSOCIATE FELLOWS.

[Limited to Fifty, of whom Twenty may be Foreigners.]

ELECTED

1873. Acland, Henry W., M.D., F.R.S., Oxford, England.
1876. Barker, Fordyce, M.D., New York.
1877. Barnes, Robert, M.D., London, England.
1876. Bigelow, Henry J., M.D., Boston, Massachusetts.
1876. Billings, John S., M.D., U.S.A., Washington, District of Columbia.
1876. Bowditch, Henry I., M.D.. Boston, Massachusetts.
1865. Butcher, R. G. H., M.D., Dublin, Ireland.
1877. Byford, William H., M.D., Chicago, Illinois.
1877. Chaillé, Stanford E., M.D., New Orleans, Louisiana.
1876. Clarke, Alonzo, M.D., New York.
1876. Comegys, C. G., M.D., Cincinnati, Ohio.
1876. Corson, Hiram, M.D., Norristown, Pennsylvania.
1876. Davis, N. S., M.D., Chicago, Illinois.
1876. Donaldson, F., M.D., Baltimore, Maryland.
1883. Fayrer, Sir Joseph, M.D., LL.D., F.R.S., London, England.
1878. Fothergill, J. Milner, M.D., London, England.
1876. Green, Traill, M.D., Easton, Pennsylvania.
1883. Heath, Christopher, Esq., F.R.C.S., London, England.
1874. Jackson, J. Hughlings, M.D., London, England.
1876. Johnson, George, M.D., F.R.S., London, England.
1876. Johnston, Christopher, M.D., Baltimore, Maryland.
1876. Jones, Joseph, M.D., New Orleans, Louisiana.
1876. King, James, M.D., Pittsburg, Pennsylvania.
1876. Kinloch, R. A., M.D., Charleston, South Carolina.
1877. Lister, Sir Joseph, Bart., LL.D., F.R.S., London, England.
1865. Macleod, G.H.B., M.D., Glasgow, Scotland.
1876. Moore, E. M., M.D., Rochester, New York.

ELECTED

1876. Mowry, R. B., M.D., Allegheny City, Pennsylvania.

1873. Ogle, John W., M.D., London, England.

1874. Paget, Sir James, Bart., D.C.L., LL.D., F.R.S., London, England.

1876. Pollock, A. M., M.D., Pittsburg, Pennsylvania.

1876. Porcher, F. Peyre, M.D., Charleston, South Carolina.

1869. Valcourt, Th. de, M.D., Cannes, France.

1857. Valery, Gaetano, M.D., Florence, Italy.

1861. Velasco, Pedro Gonzales, M.D., Madrid, Spain.

CORRESPONDING FELLOWS.

ELECTED

1880. Carrow, Fleming, M.D., Canton, China.

1880. Chiara, Domenico, M.D., Milan, Italy.

1886. Dey, Kanny Loll, M.D., Calcutta, India.

1885. Rendu, Jean, M.D., Lyons, France.

1880. Wasseige, Adolph, M.D., Liège, Belgium.

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LIST OF ILLUSTRATIONS.

NOTICE.

The present volume of Transactions contains the papers read before the College from November, 1884, to June, 1886, inclusive.

The Committee of Publication thinks it proper to say that the College holds itself in no way responsible for the statements, reasonings, or opinions set forth in the various papers published in its Transactions.

[Volume VIII. of the Third Series of the Transactions has been edited by the Recorder, Dr. J. Ewing Mears.]

This Indenture made the twelfth day of November in the year of our Lord one thousand eight hundred and eighty-five (1885) *Between Helen C. Jenks, widow of William F. Jenks*, Doctor of Medicine, late of the city of Philadelphia, hereinafter called party of the first part, and *James H. Hutchinson*, Doctor of Medicine, *James V. Ingham*, Doctor of Medicine, and *William S. W. Ruschenberger*, Doctor of Medicine, all of the said city of Philadelphia, hereinafter called parties of the second part. *Whereas*, the said Helen C. Jenks is desirous of giving the sum of Five Thousand Dollars to found a Memorial of her late husband, the said William F. Jenks, M.D. *Now this Indenture witnesseth* that the said Helen C. Jenks for and in consideration of the premises and of the sum of One Dollar to her in hand paid by the said parties of the second part hereto and for the purpose of founding a Memorial aforesaid, doth hereby give, transfer, assign, and set over unto the said James H. Hutchinson, James V. Ingham, and Wm. S. W. Ruschenberger, their successors and assigns the sum of *Five Thousand Dollars* upon the following uses and trusts and under and subject to the following limitations, restrictions, and conditions, namely, that is to say, *First*, In trust to invest and from time to time reinvest the said sum of Five Thousand Dollars as they the said parties of the second part hereto may think fit, by and with the consent of a majority of the Finance Committee for the time being of the College of Physicians of the city of Philadelphia. *Second.* In trust to collect and apply the income of said investment or investments to the payment of a Prize which shall be awarded once in every three years from the first day of January, 1886, to the author of the best dissertation upon Obstetrics, or the Diseases of Women, or the Diseases of Children. *Under and subject* nevertheless to the following restrictions, limitations, and conditions: *First.* The President of the College of Physicians of the city of Philadelphia shall have authority with the advice and consent of the Trustees under this deed for the time being to appoint a Committee to be called the *William F. Jenks Prize Commitee*, which Committee shall have full power to arange all details connected with the selection

of subjects for dissertation or reward, and also for the awarding of prizes or rewards, as provided for in this deed. *Second.* The Trustees under this deed for the time being shall have full control of all matters of finance and expense not hereinbefore provided for. They shall, also, have power to regulate the amount of the prize or reward according to their discretion, and in case of no award they shall have power to add the accumulated income to the said principal sum of Five Thousand Dollars, or in their discretion to apply the said income to a publication fund as hereinafter mentioned. *Third.* The Trustees under this deed for the time being can in their discretion publish the successful essay or any paper written upon any subject for which they may offer a reward, provided the income in their hands may in their judgment be sufficient for that purpose and the essay or paper be considered by them worthy of publication. If published, the distribution of said essay or paper shall be entirely under the control of the said Trustees. In case they do not publish the said essay or paper, it shall be the property of the College of Physicians of the city of Philadelphia. *Fourth.* The Chairman of the Finance Committee of the College of Physicians of the city of Philadelphia for the time being shall be ex-officio one of the Trustees under this deed. In case of a vacancy caused by the death or resignation of the remaining Trustees, the Council of the College of Physicians of the city of Philadelphia shall have power to fill such vacancy by a majority vote at an election held at a regular meeting of said Council at which two-thirds of the said Council shall be present, and this power is to extend to the filling of all vacancies in the Trusteeship as long as the said trust may exist. *Fifth.* The Trustees for the time being shall have authority either to lengthen or shorten the interval between the awarding of the prize or reward in case they think it advisable so to do, and they also shall have authority to use the income above mentioned for some other memorial as a prize or reward as aforesaid, which prize or reward must always be for some subject connected with Obstetrics, or the diseases of women or the diseases of children; such change or substitution, however, is not to be made without the unanimous consent of the Trustees and without the written consent of two-thirds of the Council for the time being of the College of Physicians of the city of Philadelphia, said change or substitution, however, not

to affect or change any of the other provisions of this trust except so far as the same be inconsistent therewith. *Sixth.* The Committee of Award, appointed as aforesaid, shall have authority to refuse a prize or reward to an essay or paper or to any substitution thereof in the nature of a reward even if they consider the same the best offered provided they do not think it worthy of the prize. *Seventh.* The successors to the Trustees herein named shall have all the powers and authorities given to the parties of the second part hereto.

In witness whereof the said Helen C. Jenks hath hereunto set her hand and seal the day and year first above written.

HELEN C. JENKS [SEAL].

Sealed and delivered in the presence of us:

WM. H. MYERS,
WM. B. ROBINS.

On the twelfth day of November, Anno Domini 1885, before me the subscriber, a Notary Public of and for the County of Philadelphia, personally appeared the above named *Helen C. Jenks*, and in due form of law acknowledged the above Indenture to be her act and deed and desired the same might be recorded as such.

Witness my hand and official seal the day and year aforesaid.

[SEAL] WM. H. MYERS,
Notary Public.

We hereby acknowledge the receipt of the sum of five thousand dollars from the said Helen C. Jenks and accept the foregoing trust. Dated this twelfth day of November, Anno Domini, 1885.

Witness:
WM. B. ROBINS.

JAMES H. HUTCHINSON,
JAMES V. INGHAM,
W. S. W. RUSCHENBERGER,
Chairman of the Finance Committee of the College of Physicians of Philadelphia.

Recorded in the office for recording of deeds, etc., in and for the city and county of Philadelphia, in Deed-book G. G. P. No. 82, page 348, etc.

Witness my hand and seal of office this fourteenth day of November, A. D. 1885.

GEO. G. PIERIE,
Recorder of Deeds.

MEMOIR

OF

JOHN LIGHT ATLEE, M.D., LL.D.

BY

D. HAYES AGNEW, M.D.

[Read February 3, 1886.]

On the fifth day of October, 1885, might have been seen one of the most notable gatherings that had ever taken place in the city of Lancaster. This assemblage embraced members of the learned professions and citizens of every rank and condition of life. All gathered together in order to participate in the last sad rites of burial to one of their most distinguished townsmen—Dr. John Light Atlee. And how preëminently fitting this pageant of love and respect! Here was a man who had seen portions of four generations of his fellows come upon the stage of life, play their varied parts in the great drama of humanity, and then silently pass out of sight. Here was a man who had participated more or less actively in all the chief and moving questions of beneficence, education, and government which belong to the body politic of a populous city. Here was a man who had been admitted for an exceptionally long period of time, in the exercise of professional duties, into the innermost sanctities of a vast number of households. Ever a conspicuous figure among his fellow-men, known to every dweller in the city of

his birth, no wonder that his mortal remains should command such reverent respect before being committed to their kindred dust.

Dr. John Light Atlee was of English origin, and came of a long line of distinguished antecedents, dating their lineage back in an unbroken line anterior to the time of Charles II., and most of whom had filled places of great responsibility and honor. His great grandfather, William Atlee, of Fordhook House,[1] England, in the parish of Acton, came to this country in 1733, as private secretary to Lord Howe, at the time the latter entered upon his duties as Governor of Barbadoes. William Atlee's wife was the daughter of an English clergyman and a cousin of William Pitt. The grandfather of Dr. Atlee, William Augustus Atlee, first son of William and Jane Atlee, was born in Philadelphia. He studied law in the office of Judge Shippen, was admitted to the Lancaster bar, rose to eminence, and in 1777 became First Associate Judge of the Supreme Court of Pennsylvania. He was an ardent and patriotic supporter of the Colonial cause, and after the establishment of our independence was appointed President Judge of the First District Court, which embraced in its jurisdiction the counties of Chester, Lancaster, York, and Dauphin; and while still in the exercise of his judicial functions fell a victim to yellow fever. William Pitt Atlee, son of Judge Atlee and Esther Sayre, married a daughter of Major John Light, and of the six children resulting from this union, the subject of the present sketch, Dr. John Light Atlee, was the eldest. He was born November 2, 1799, in the city of Lancaster. His academic education was received at the academy of Gray and Wiley, a school for a long time famous in the city of Philadelphia, both or its thorough curriculum of instruction and the strictness of its discipline. Dr. Atlee's medical education, hastened by the early death

[1] In a drawing-room, in the more modern portion of this family homestead, wainscoted in oak in the year 1500 by one William Atlee, Ada, the daughter of Lord Byron, whose wife then resided there, was married to Lord King.

of his father, began in 1815, in the office of Dr. Samuel Hulmes, of Lancaster. In 1817 he entered the medical department of the University of Pennsylvania, from which institution he received the degree of Doctor of Medicine in 1820. After graduation, Dr. Atlee returned to the city of his birth and there commenced the practice of his profession. Two years later, March 22, 1822, he married Sarah H. Franklin, daughter of the Honorable Walter Franklin, a distinguished jurist, and for a long time President Judge of the courts of Lancaster and York Counties. Mrs. Atlee was a woman of great nobility and force of character, and adorned every relation of life with a rare grace and dignity of manner, until called away by the hand of death fifty-eight years after her marriage. The children who survive these parents are Dr. Walter F. Atlee, of Philadelphia, the well-known surgeon and honored Fellow of the College of Physicians, William Augustus Atlee, a prominent member of the Lancaster bar, and Miss Anne Franklin Atlee, whose long and untiring devotion to her parents entitles her to King Lemuel's commendation, "Many daughters have done virtuously, but thou excellest them all." A third son, Dr. John L. Atlee, Jr., a physician greatly endeared to all who knew him, died in the early years of his manhood.

From the time of his graduation, in 1820, until within a few days of his death, Dr. Atlee was actively engaged in the duties of his profession. A few months before his demise he had an attack of facial paralysis, induced, it is believed, by indiscreet exposure. This was the signal for a halt. Up to this time, the multiplying years had dealt so generously with the man that he seemed to possess a spring of perpetual youth. His tall, erect, agile figure, his quick, elastic step and sunny face betokened no abatement of bodily force. His mind never for a moment experienced even a partial eclipse, but continued to act with its wonted energy, and in a short time, despite a moderate physical disability, he was again engaged in professional

work. The final summons came in the form of an attack of pneumonia, and on the afternoon of October 1, 1885, the gentle spirit of Dr. Atlee left the clay tenement which it had worn and animated, for well nigh eighty-six years, for another and a better world.

In the contemplation of a life like that of the subject of the present sketch, it may be viewed from three distinct points of observation, as a citizen, as a physician, and as a man, for we find three distinct sides to his character. While it is true that the professional man must ever be loyal to his calling, yet the common, public interests of the community in which he lives, and of which he is an integral part, cannot be ignored. There are men who possess that singular flexibility and versatility of the mental constitution which enables them to project their personality into many and diverse lines of benevolence or education, becoming in each a recognized force, and yet at the same time carry along all the routine of their own professional work with scrupulous exactitude. Such men, however, are insular. Dr. Atlee was one of that select and unique class. When the effort was made to introduce the system of general or free school education which had been created by our State Legislature in 1838, it was met by a formidable opposition, especially among the Germans, who formed a large element of the population in Lancaster County. These people still clung to that school system which existed under the Act of 1820. When the time arrived for the substitution of the new method of instruction for the old, Dr. Atlee appears as the public man, and it was largely through his personal influence and individual efforts with the people among whom he lived, that their prejudices were overcome, and the system of free schools successfully inaugurated. Nor did his interest in this great scheme of education cease with its establishment. He became the chairman of the superintending committee, and for forty years acted as a director of the School Board of the county. In founding the Normal School at Millersville, he exhibited the same

untiring zeal. The resolutions of the School Board of Lancaster City, passed after his death, fully attest the value of his services in the cause of education. Again, as a trustee, no one was more active in the organization of Franklin and Marshall College, in which institution he was chosen Professor of Anatomy and Physiology, and from which he received the degree of Doctor of Laws.

Possessing a rare faculty for management, his aid was sought in other fields than those of education. He was one of the managers of the House of Refuge in this city; he was appointed a trustee in the Bishop Bowman Church Home, in Lancaster, acted as president of the Board of Trustees of the Home for Friendless Children, and held the same position in the Board of Managers for the State Lunatic Asylum at Harrisburg.

In turning now to the medical career of Dr. Atlee, we find in him a remarkable and harmonious combination of all those qualities which go to make up a good and great physician. He was a life-long student, a close observer, bold and self-reliant without being rash, honorable in professional intercourse, the implacable foe to all forms of charlatanry, or doubtful methods, whether in or out of the profession, and withal possessed a sweet, gentle disposition, combined with a cheerful and dignified deportment, eminently calculated to inspire confidence and hope in the sick, and to command respect among his medical brethren. Ever active in all that concerned the interest and advancement of medicine he was among those who, very early in the history of medical organizations, discerned the importance of centralizing professional power, not only as a scientific measure, but as calculated to establish a kindly reciprocity of feeling among physicians, and thereby elevate medicine in the public estimation. Accordingly, we find Dr. Atlee a leading force in founding the Lancaster County Medical Society in 1844, over which body he twice presided as its president. He was one of those also who, in 1848, were prominent in the organization of the State Medical Society,

and in 1857 was honored by being chosen the presiding officer of that body. When the scheme to organize a national representative body of physicians (the American Medical Association) was conceived, in 1847, we find Dr. Atlee's name among its founders. In 1868 he was elected vice-president, and in 1882 president of this organization. In 1877, in recognition of his high professional reputation in the department of abdominal and pelvic surgery of the female, Dr. Atlee was elected an honorary member of the Gynecological Society of Boston. He was also an Associate Fellow of the College of Physicians of Philadelphia.

The most noteworthy event, however, which towers conspicuously above others in the surgical career of Dr. Atlee, was the revival of the operation of ovariotomy. As new discoveries, or methods of managing disease, become established and made common property, we soon forget or underestimate the merits of the originator.

The man, for example, who, without a single precedent to illuminate the path of experiment except his own singular faith, pressed ether to the stage of profound anesthesia, must have been animated by nothing short of a God-given courage, and though no monument of brass or of marble records the name of Morton or Wells, yet until the end of time humanity will be their debtors. And so with regard to ovariotomy. When Dr. Atlee conceived the idea of reviving this operation (in 1843), there was nothing to lighten his path, save the cases of McDowell, dating back to 1809, and these almost forgotten; on the contrary, there was everything to intimidate and discourage one less bold. Men occupying the highest positions in the profession as writers and teachers, on both sides of the Atlantic, men who had for years moulded medical and surgical thought like plastic clay, had spoken their condemnation of the procedure, some of them in no charitable terms; yet, this man with a dauntless courage, the offspring of inborn convictions, steps forward out of the rank and file of imperious authority, assumes the respon-

sibility of being singular and antagonistic, and inaugurates anew an operation which every succeeding year delivers an army of women from death and lays the world under tribute to his genius. Nor do the far-reaching results of this surgical venture terminate with ovariotomy, it was the germinal promise of all that rich harvest of abdominal surgery which has marked the progress of our art in these later years. Indeed so wonderfully articulated and dependent are the outgrowths of these seminal thoughts, that it is impossible to predict what avenues of surgical venture are yet to be opened. They are like some of those divine epigrams, "Who is my brother?" "No man liveth to himself," "No man dieth to himself," the profound depths of application of which have never yet been plumbed by the measuring instruments of man. Ah, yes, in the magnitude and resistless sweep of the river we too often forget the spring-heads, the sources of its strength and power! To America belongs the honor of that great boon which renders painless the knife of the surgeon and the throes of maternity; to America belongs the seed-thought of subcutaneous osteotomy, and to America belongs the glory of ovariotomy.

No man perhaps has more successfully filled the various departments of medicine than Dr. Atlee. Almost half a century of his professional career was lived before the specialties of medicine had attained any very marked public recognition. In over two thousand operations of which he has left us a record are embraced every manner of surgical procedure, covering every branch of the now specialized surgery, and these followed by a success which compares not unfavorably with the leading operators of the world.

On the 23d of January, 1822, he operated for fistula in ano, and on the 9th of September, 1885, he performed tracheotomy. These operations were almost sixty-four years apart. Six times in his eighty-third year, and three times in his eighty-fourth year, he made the operation of ovariotomy. The cunning had not left his hand, nor had a shadow crept across his mind. What an immense

well-spring of vitality must have been that, which could so continue to energize the evening life of this man—*quando ullum invenient parem?*

As an obstetrician his experience, under the circumstances of locality, was exceptional, having attended three thousand women in childbirth, and as a practitioner of medicine he enjoyed an extended and unrivalled domain of patronage. It was this great wealth of experience gleaned from so wide a field of medicine, that commanded the confidence of his professional brethren, who, from all parts of the land, sought his counsel and aid.

There is something after all, besides grace of manner, force of intellect, and public spirit, which is necessary to round out character to its full and just proportions, and that is a conscious sense of moral responsibility. It has been said that the physician, constantly absorbed with the phenomena which largely concern the physical nature of man, is prone to forget the ego, the spiritual, indestructible essence which survives decay, and for which the body is only a convenient environment during the probationary period of existence. I cannot believe that, in any general sense, the charge of materialism can be justly made against our profession; or that medical men, who, as a class, have enjoyed the benefits of a liberal education, and therefore, it may be assumed, have some knowledge of the higher domain of philosophy, can believe that either this body instinct with deity and law, or the principle of consciousness and thought which inhabits it, when the nexus is dissolved, sinks into a gulf of annihilation. Be this as it may, Doctor Atlee, with all his devotion to the study of the human body and its diseases, ever maintained that mental equipoise which moored him to the great verities of the future, and left no place in his mind or heart for the shallow vagaries which ever and anon rise on the sea of speculative thought. Very early in life he connected himself with the Episcopal body of Christians, was for forty years a senior warden in that church,

attending with uniform regularity her ecclesiastical courts, and always taking an active interest in all the philanthropic activities of this religious organization. Singularly pure and exemplary both in his private and public life, there was yet nothing of austerity in his demeanor. A man of great simplicity and honesty of character, he hated shams. On one occasion when asked by a clergyman of his church, who, on a preceding Sabbath had performed the service in an assumed affectation of voice, how he liked the intonation, the Doctor replied with considerable feeling, "if there is any time in this world, sir, when a man ought to be natural it is when he is addressing the Supreme Being."

It is said, I think, by Macaulay, that no power is more formidable than that of being able to make men ridiculous. When a man ahead of his day, like Fulton, like Jenner, like Paré, after announcing some fact out of the common experience, has been made the mark for taunt and ridicule, and when time, that great adjudicator and vindicator of the real and the true, places that fact beyond the region of controversy, how great is the temptation to retaliate, and yet after ovariotomy by the common consensus of the medical world became an established operation, no man ever heard Dr. Atlee say one unkind word toward, or use his commanding vantage-ground to cover with confusion his detractors. Sixty-five years of public life, as physician, citizen, and Christian gentleman, and in each of these spheres blameless; what a royal legacy to leave behind! "A good name is rather to be chosen than great riches, and loving favor rather than silver or gold." There is a philosophy which asks in tones of horrid despair, "Is life worth living?" a philosophy the monuments of which are countless suicides and unhonored graves. Here was a life that was worth living, and the hand of love could write no more appropriate epitaph than those simple words which are reverently inscribed on the gravestone of our distinguished and beloved associate, "*Pertransivit Benefaciendo.*"

BIOGRAPHICAL NOTICE

OF

GEORGE HAMILTON, M.D.

By

JOHN ASHHURST, Jr, M.D.

[Read June 2, 1886.]

The career of a modest, hard-working practitioner of medicine affords ordinarily but scant material for the biographer or panegyrist, and yet the history of such a career may furnish useful lessons for posterity, and may present for imitation a worthy example, always more effective than precept. Hence the pleasant custom of this College seems reasonable and judicious, that on the death of such of its Fellows as have deserved well of their profession, the bare record of decease should be supplemented by a brief biographical notice, and by a few words of affectionate remembrance of the colleague who has gone before.

In full sympathy with this laudable custom, I have gladly accepted the duty placed upon me by our President and Censors, and beg leave to recall to you to-night the name of that good man and sound physician, the late Dr. George Hamilton, whose appearance was doubtless familiar to all who hear my voice, and who was an honored Fellow of this College for more than twenty years.

George Hamilton, as I learn from a memorandum kindly furnished me by his son, Mr. J. McClure Hamilton, was born in this

city on the 15th of November, 1808, and had, therefore, at the time of his death, nearly completed his seventy-seventh year. I can learn but little of his early life. He was of Scotch-Irish parentage, though of English ancestry, and received his education at various private schools; the name of only one of his instructors, however, is remembered—Dr. Banks, a clergyman, who taught him Latin. He does not appear to have had the advantage of collegiate training, a circumstance which only renders more creditable to himself the proficiency which he afterwards acquired in various branches of learning.

Dr. Hamilton's resolution to study medicine appears to have been determined by an accidental meeting with the late Dr. Thomas T. Hewson, a former President of this College, son of the eminent English physiologist, William Hewson, and father of Dr Addinell Hewson, a valued senior Fellow of our own day and generation. Dr. Hamilton's studies were begun under Dr. Hewson's preceptorship in 1828, and he graduated from the University of Pennsylvania in 1831, the year after Joseph Carson, William W. Gerhard, and George W. Norris, and the year before Thomas S. Kirkbride, Minturn Post, and the elder Pepper. The subject of his thesis was intermittent fever.

Entering into practice in the northern part of the city, Dr. Hamilton after a couple of years became dissatisfied with his prospects, and determined to remove to the country. He was dissuaded from this course by his preceptor, who wisely told him that in ten years he would be as prosperous in urban as he could be in rural practice. This advice, however, did not prevail, and, attracted by an advertisement of sale of the property of a Dr. Peters, Dr. Hamilton determined to establish himself at Centreville, Newcastle Co., Delaware, a post-village which, according to the Medical Directory of 1885, even at the present day supports but one physician. Dr. Hamilton was disappointed in regard to obtaining

Dr. Peters's place, in the purchase of which he was anticipated by Dr. Joseph P. Chandler; but being personally solicited to remain by influential members of the community, he, in 1833, took up his abode in Centreville, and for nearly twelve years shared with Dr. Chandler the anxieties and responsibilities of a widespread rural practice extending through all the neighboring country. The experience thus gained was subsequently of much value to Dr. Hamilton, and enabled him to see both sides of some questions which those who have practised in towns exclusively are apt to look at from but a single point of view. In 1844, being then thirty-six years of age, Dr. Hamilton returned to Philadelphia, and opened an office in his father's house, at the south-west corner of Sixteenth and Summer Streets, where he continued to live until his death. At the age of forty-one, he married a daughter of James Delaplaine, Esq., of Delaware—a happy marriage which gave him four children, two of whom, a son and a daughter, with their mother, survive him. The first-born died in infancy, and the youngest daughter perished only a few months before her father's death by a dreadful casualty which is still fresh in the memory of all our fellow-citizens.

During the time of his student-life, as I learn from his son's memoranda, and during the first years of his practice, Dr. Hamilton devoted much time to botany, and collected many specimens of our local flora in his rambles over the fields and hills bordering upon our city, and especially through the beautiful valley of the Wissahickon; his botanical studies, however, were not continued, and in his later years his collections were limited to books and engravings, of which he accumulated large stores.

"He was very fond," his son writes, "of birds and animals, and had the power to a wonderful degree of winning their sympathy. Canary birds became affectionately attached to him, and his pets would recognize his step on the stair or in the hall, and his form approaching at a distance in the street." No doubt this

genuine love for the innocent members of the lower orders of creation helped to fix his mind in opposition to the abuses of vivisection, as practised by some of its less judicious advocates.

"In a quiet way, he admired fast horses, but could never be induced to frequent race-courses. While in the country he owned a very fast pacing mare, and I am told," says his son, "that, after thoughtlessly selling her, he wept tears of remorse that he had not retained her and cared for her old age. In later years he might often be seen, a white-haired, quakerish-looking old gentleman, speeding along in the dust of South Broad Street on his thick-set and over-fat Canadian pony, vying with the well-trained trotters coming from Point Breeze Park, and frequently excelling them."

Dr. Hamilton's fondness for books has already been alluded to, and it was through a community of tastes in this respect that my personal acquaintance with him began. Book-buyers of twenty-five or thirty years ago will remember the rare opportunities of increasing their collections at moderate cost, at that time presented by the frequently recurring book-sales at what was then the "new" auction store of Moses Thomas & Sons, on South Fourth Street, where the timid or sluggish bidder was encouraged, and the whole company kept in good humor, by that prince of "criers," the late Thomas F. Bell. It was on the morning of one of these sales, while examining the prospective purchases of the evening, and finding the coveted treasures all too numerous for the modest resources of my purse, that I first exchanged words with Dr. Hamilton. Seeing me turning over with loving hands the folio pages of Bonetus's great work, the *Sepulchretum*, the Doctor propounded to me the question whether I regarded it as of equal authority with the *De Sedibus et Causis* of Morgagni. My answer, in the negative, appears to have been judicious; at least it represented, I believe, the comparative estimate of these authors then and now held by those best qualified to form an opinion on the subject, and it laid the foundation of a permanent

friendship with my interlocutor, which grew with advancing years, and only ceased with his death.

But it was not only at auction sales that Dr. Hamilton made his purchases:

"Every bookstand and old nook," writes his son, "was ransacked for musty volumes. He had not the means to indulge his fancy for the more expensive editions, but he was enabled, by discriminating purchases, to bring together some fourteen or more thousands of interesting volumes, among which there were many of considerable rarity and worth. The afternoons and evenings were given, when possible, to examining, scanning, and reading the purchases of the preceding day, and in his devotion to his hobby he forgot the cares of his profession and of his life. Owing to certain family arrangements he was unable to classify and place his library as he wished, so that the books were consigned pile by pile to each room in succession, and in time began to encroach so much upon the space of the house, that it became a question whether they would not ultimately expel the inmates."

Dr. Hamilton was a good French, German, and Italian scholar, and was especially an admirer of the *Divina Commedia* of Dante. He was fond of music and art, and not insusceptible to the charms of the drama, though never much of a play-goer. After having seen Rachel, he was never known to enter a theatre but once, on the occasion of one of Mr. Irving's representations, not wishing probably to have the impression made by those great actors dimmed by the less brilliant efforts of other performers.

In art he was particularly fond of line-engraving, and, as his means permitted the gratification of this taste within reasonable limits, he from time to time acquired specimens of various masters in this department, until he had accumulated between three and four thousand prints, including some fine impressions of the works of Edelinck, Wille, Audran, etc.

As a physician, Dr. Hamilton was a sound, careful, and judicious practitioner, and much beloved by his patients. It was my privilege to meet him occasionally in consultation, and I know of no one who united more happily than he did that "*tenderness* with *firmness*, and *condescension* with *authority*," which our code of ethics tells us will serve to "inspire the minds" of patients "with gratitude, respect, and confidence." "He was well known," writes Prof. Henry H. Smith, "as a sound practitioner. . . He had a sound, calm judgment in disease." He was long closely associated in practice with the late Dr. Gebhard, who, elected in 1828, was a Fellow of this College for more than forty years. Though, as already mentioned, fond of horseback riding for pleasure, Dr. Hamilton generally went on foot to see his patients, or, if they were very distant, used the public cars. He was always very moderate in his professional charges, and he was especially considerate of the poor. He became a Fellow of this College in April, 1865.

Dr. Hamilton was a careful though not a voluminous writer. Those of his productions which attracted most attention were probably his address before the Philadelphia County Medical Society, of which he was President in 1868, and several papers, some of which appeared in our *Transactions*, on the etiology of typhoid fever, and on the propriety of regulating the practice of vivisection by legal enactment. Probably the last fruit of his pen was a touching and loving memorial of his beloved daughter, whose tragic death has already been referred to.

As a man and as a citizen, Dr. Hamilton's influence and example were always on the side of morality and virtue. While not a believer in the essential necessity and duty of total abstinence, he was always a strong advocate of temperance, and while living at Centreville headed a movement to suppress certain taverns the maintenance of which endangered the sobriety of the neighborhood. He was, writes Dr. Hatfield, "a gentleman of high sense of honor and

stern integrity, accompanied with a cultivated and well-balanced mind," and yet with a "quiet, easy, and affable manner, as if he had no conception of his real strength and power." "I loved him," writes Dr. Hiram Corson, "for his sincerity, truthfulness, and devotion to what seemed to him right."

Though always quiet, and usually even grave in his demeanor, Dr. Hamilton was by no means destitute of the sense of humor, and could jest on occasion, as is witnessed by an anecdote told of his Centreville life. During the time preceding the signing of the Webster-Ashburton treaty of 1842, when feeling in this country ran high against Great Britain on account of our disputed boundary line, as it did again four years afterward when a favorite watchword with the belligerent party was "fifty-four forty or fight," the rumor prevailed in Centreville that a British invasion was imminent, and that all citizens would probably be called upon to defend their own homes and firesides. A number of the villagers were gathered in front of the blacksmith's forge, eagerly discussing the situation, and doubtless speaking very brave words, when Dr. Hamilton quietly rode up to them, and said, in his most serious tones: "I have just seen the Quartermaster, and he is coming to-morrow to enroll you all." Anxiety and consternation seized the group, and each one, pressing forward, eagerly presented a sore finger, lame toe, or other ailment, for which he begged the Doctor to grant him a certificate of disability which might exempt him from service. In a few hours the whole village was plunged in gloom and misery, and for very pity's sake the Doctor had to confess that his interview with the Quartermaster had been imaginary.

In his private life, Dr. Hamilton was happy. Blessed with a devoted wife, he in turn ever showed himself a tender and loving husband. Considerate and affectionate as a father, he was devoted to the interests of his children, sharing their sports while they were young, and ever endeavoring to promote their welfare, even at the

cost of his own comfort, as they grew older. He educated them liberally, and for three years allotted a considerable portion of his modest income to defray the expenses of his son, now a distinguished artist, then studying in Europe; for this expenditure, however, I personally know that he was well repaid by the gratification afforded him by his son's success in his chosen vocation.

In the early morning of February 21, 1885, by a destructive fire suddenly breaking out in a dwelling house not five squares distant from this hall, four lives were lost, and two of our Fellows were bereaved of their children. Dr. Hamilton's younger daughter, the husband of his elder daughter, his grandchild, and a maid-servant, were killed, and the life of the elder daughter was saved as by a miracle. I have before me a letter written, but not mailed, to his valued friend Dr. Hiram Corson, dated the day before the occurrence of this dreadful tragedy; and the heart-broken postscript, tremulously written with lead-pencil, added when the unsent letter was found a day or two afterward. In his great affliction Dr. Hamilton was sustained and soothed by the loving companionship of his wife and surviving children, by the sincere and heartfelt sympathy of his wide circle of friends, and, above all, by his firm faith in the truths of religion, as revealed in the New Testament of our Lord and Saviour, Jesus Christ. But though the spirit was sustained, I think that, physically, Dr. Hamilton never recovered from the shock of that February morning. I met him in our lower hallway not many weeks before his death; there were the same friendliness of manner, the same kind and gentle smile, but it was obvious that the old man's strength was broken, and that his days on this earth were numbered. Returning from church on Sunday evening, October 25, he was seized with his last illness, lingering without pain for a little more than four days, and giving up his soul to his Maker on the morning of October 30. The cause of his death was hemiplegia.

A good man—*integrum*, *probum*, *industrium*—who proved his love to God by doing justly and showing kindness to his fellow-men: *Neque enim hoc homine sanctior, neque probior, neque in omnibus officiis retinendis diligentior esse quisquam potest.* His happy, useful life, his peaceful death, verified the true saying of Philo:

Θεῷ δουλεύειν οὐκ ἐλευθερίας μόνον, ἀλλὰ καὶ βασιλείας ἄμεινον.

ADDRESS

DELIVERED AT THE CLOSE OF HIS FIRST TERM OF OFFICE,

By

J. M. DA COSTA, M.D., LL.D.,
PRESIDENT OF THE COLLEGE.

[Delivered February 4, 1885.]

Fellows of the College:

In addressing you to-night in accordance with the law requiring the President to make an annual address, my task is limited by circumstances. The one who last year spoke to you, gave you so excellent a history of the College that it would be mere waste of time to try to add anything to what has been so admirably set forth; and it is the very excellence of the work which compels me to limit myself to a mere discussion of our condition and needs. I shall, then, call your attention chiefly to the business of the College; inquire what its position, its wants, and how we can all best work together to increase its usefulness and power.

The activity of the College has continued unabated. The meetings have been largely attended, the number of papers presented has been considerable, their contents valuable, and many interesting remarks have been called forth by their discussion. Some of the contributions are elaborate and admirable, and of a character which will compare not unfavorably with the best in the transactions of learned societies elsewhere. To specify might be looked upon as invidious, but a very brief examination of Vol. VII., the volume ust published, will show papers which will be quickly recognized as

permanent additions to medical literature. It is not only in size but in quality that our volume is constantly gaining.

In part this gratifying state of things is due to the aroused interest in our meetings. But it has received powerful aid from the wise and energetic action of the Publication Committee. Every communication is at once put to press and copies are sent to a number of prominent journals, in advance of the papers being collected into a volume. This insures their speedy notice and wide diffusion, with their subsequent collection in a permanent form in the College transactions. Extra copies, too, are at once obtainable. There is, indeed, to the author no mode of publication more advantageous than that offered now by the College; to make it, however, of the greatest use to both the College and the author, the manuscript, or at least a summary of it, should be promptly furnished.

Yet there are some thoughts in connection with our meetings looking toward their still greater scientific interest, which I venture to lay before you. A fuller discussion of the papers would, I believe, do much to aid us. This can be accomplished if summaries of the chief papers to be read were sent in advance to such members as are known to be specially interested in the subject. The author could easily furnish such a summary, and it would be but a trifling expense to print it. Moreover, the employ of a shorthand reporter at the meetings, under the direction of the Recorder, would insure fulness and accuracy, and enable the debates to be at once printed after the reports had been submitted to the speakers. There will be some expense in this, but a Fellow of the College has agreed to furnish the means for trying the plan during this year, if sanctioned by the College. Another means of adding interest to the meetings would be to fix occasionally an evening for a debate on subjects actively engaging professional thought.

The growth of the library is a subject of just pride. From the two hundred and fifty books which were moved in a small cart to this building in 1863, it has increased to over thirty-two thousand volumes, most of them in excellent condition, and some of them very rare. How much of this increase is due to the steady generosity of your last President, of whom all wished the continuance in office, and whose wise words would have been received by you most gladly

at this meeting to-night, we all know. His name is linked to the history, to the usefulness of this College. His spirit will continue to actuate us. The good he has done will increase as the College roll grows with years. The benign face that looks down on us in this hall, from among our list of worthies, will see hundreds, reading, writing, analyzing, and hear the blessings on the generous scholar who lightens their toil by giving them these great opportunities.

Five thousand eight hundred and twenty-nine volumes were added to the library in 1884, and about twenty-two hundred duplicate volumes have been sold. While Dr. Samuel Lewis is still the most constant donor, others have in the last year contributed very largely, and the College has to thank Dr. Stillé for a splendid gift of nearly one thousand books, not one of which it previously possessed. There are now on the tables upward of two hundred and fifty various current American and foreign journals, and in a special room the large surgical library of Dr. S. D. Gross has been placed on deposit by the Academy of Surgery, and is in free use.

The Mütter Museum continues to be well managed by the committee having it in charge. Owing to the character of the bequest, which permits the funds to be employed for certain purposes only, the committee finds itself with a plethora of means. By a recent decision of the Court, it believes itself authorized to contribute five thousand dollars toward the enlargement of the College building, the additional story which is contemplated being chiefly designed for the accommodation of the Museum. Will the committee permit the suggestion, whether it be not possible for them in the future to apply some of their income to a laboratory of histology and pathological research? In the new building a room for this could be provided by the College. Surely the good which would come would be far greater than from the accumulation of specimens and models; and it is not too much to say that had the founder of the Museum lived in our generation, his broad spirit would have made him gladly accept the means of adding value to the collection which bears his name, while increasing and diffusing knowledge productive of great results.

Our building, as it stands, is incomplete, and we are outgrowing it. The library, the museum, and other objects demand more space. The College has fully recognized this in the attempt which is being

made to collect subscriptions for an additional story. The energetic committee having the matter in charge have done extremely well. But I call on the College to aid them in every way. We want the building much; but we must try and erect it without incurring debt. With the sum decreed from the Mütter Museum fund, and accepted by the College, we have about two-thirds of the amount required. Let us endeavor to get the rest speedily, and then build free of embarrassment.

An additional reason for the effort to complete the structure according to the original design is the approaching centennial of the existence of our College. In 1887 we expect to celebrate it, and to entertain a number of guests from abroad. We Fellows of the College, all members of the profession, every citizen of Philadelphia, will be alike interested in having a building showing our collections to advantage, one which shall be a credit to our commonwealth and our country.

This College occupies an exceptional position. Its age, the distinction of the original Fellows, and of those who followed them, the value of its collections, and their union in a permanent building, make it a well-known centre in professional circles. But does it occupy the position it should with the public? It is, I believe, neither rightly known nor sufficiently appreciated by them. It has not the influence to which it is justly entitled. This state of things is somewhat, I think, our own fault. We are chiefly a scientific institution, but we ought also to make ourselves heard on the questions of the day concerning which our profession is best qualified to judge. If we did so oftener, we would be appealed to as a final authority, and the community would learn to look to us for guidance in many things. This would give the College a hold on the public which would lead to general interest in its doings and its extension.

From the point of view just mentioned, the recent appointment of the Cholera Committee is a step in the right direction, and there are many other subjects which legitimately are ones for us to act in.

As a means of interesting the community in us, and of making for ourselves a more commanding position, we ought to consider whether an occasional public reception at which objects of scientific interest should be exhibited, would not be most useful. The

munificence of one of our Fellows has furnished a fund for the entertainment chiefly of distinguished strangers. One of these entertainments was held during the past year on the occasion of the meeting of the Association for the Advancement of Science. That gatherings such as these will also aid in making known our institution, and interesting men of liberal culture there can be no doubt.

Our permanent fund is increasing but slowly, and is chiefly fed by life memberships. We should, even if temporary retrenchment be requisite, endeavor yearly to add to it. It is wiser for the College not to spend its full income, but to leave something annually to go to this permanent fund. It would be far better for our future. In science let us be progressive; in finance, conservative.

The advantage of this policy is that it will ultimately make it possible to reduce our annual fees. I think we should aim at their reduction. Young, struggling men of decided ability, but dependent upon the receipts from a scantily paid profession, should not be deterred from coming among us. From an institution with such benefits to confer as this no man must be kept away by want of means.

The College is, however, growing. During the last year, twenty-three Fellows were added to its roll, the largest number in any one year since its foundation. Death has removed five of our resident Fellows: Samuel D. Gross, Robert E. Rogers, Caspar Morris, Frederick C. Sheppard, and Charles T. Hunter. The great surgeon who heads the list has received his meed of praise from many pens. A Titan in work, we shall not soon see his equal; nature does not produce men in his mould frequently. Robert Rogers, the eloquent Professor of Chemistry, leaves behind him many pupils to remember his impressive teachings. Caspar Morris died in voluntary retirement. He had for some years abandoned the active pursuit of his profession, and to many here he was personally unknown. But in more than one home in Philadelphia, his pure life is often alluded to, his memory reverenced. Not the large and influential practice he enjoyed spoiled in him the painstaking, sagacious physician, and there can be no greater praise to anyone than that his bearing among

the sick recalls the ardent interest, the self-sacrificing devotion, the tenderness of this beloved physician.

These men had done their lifework, and reached advanced years. Not so with the two other Fellows whose death we deplore. Sheppard died of consumption at the age of twenty-seven, when he was just beginning to show qualities which would have led to marked professional preferment. Hunter, older by about thirteen years, and Fellow of the College since 1871, had in him the material of which distinguished surgeons are made. Of excellent judgment, cool, dexterous, a finished anatomist, his advice was often sought by his contemporaries. It has been my own lot to be associated with him in cases of great gravity, in which important surgical proceedings became necessary, from which I carried away a profound admiration for the admirable skill of the promising surgeon. His career ended three years before his death, and weary, long years of suffering they were.

Such is a sketch of the events concerning the College during the past year, with a consideration of some matters looking to its further development.

Fellows of the College: Our motto, *non sibi sed toti*, is the best guide to our action in the future. It binds us to fellowship in intercourse, in humanity, in science. It pledges us to preserve the high tone which our distinguished forefathers in the profession impressed on our institution, and to transmit it unaltered to our successors. It excites us not to rest until we have permanently enriched our science. It makes the search after truth the object that unites us; the good of all the only good which will merit and will receive honorable recognition here.

REMARKS UPON CHRONIC CONTRACTED KIDNEY WITH NORMAL URINE:

Acute Gouty Dementia with a Perforating Recto-vaginal Ulcer, and Death from Sudden Pulmonic Œdema.

By

H. C. WOOD, M.D.,

CLINICAL PROFESSOR OF NERVOUS DISEASES IN THE UNIVERSITY OF PENNSYLVANIA, AND NEUROLOGIST TO THE PHILADELPHIA HOSPITAL.

[Read November 5, 1884.]

Mrs. L., whose case forms the basis of the present article, was a very intelligent lady, about forty years of age, the mother of five children, of gouty ancestry. At regular intervals of four years she was accustomed to have very violent acute attacks of gout or rheumatism associated with great systemic disturbance and depression of spirits. As the last gouty sickness was in the early spring of 1880, she was in the spring of the present year in great fear of another attack, and an attempt was made, under the direction of her medical adviser, Dr. Tomlinson, to ward off such sickness by careful diet and free horseback exercise. For the early notes of her case I am indebted to Dr. Tomlinson. An exposure to cold during the night of April 20 was followed by severe coryza, vague pains, and great hebetude, with a very pronounced desire to sleep. Even when moving about she seemed unable to keep her eyes open. Under treatment she improved temporarily. Dr. Tomlinson writes concerning this period, "I could discover nothing wrong with the urine, which

she passed in usual quantity. A week later she began to have difficulty in expressing herself; she would use irrelevant words, and then correct herself; her gait also grew uncertain, and in walking she would pitch forward as though she were going to fall. April 28, Mrs. L.'s mother died; Mrs. L. was greatly shocked, and rapidly became worse; she was greatly depressed, lachrymose, hysterical, had hallucinations, and ceased to recognize those around her. She lost her appetite, and became constipated. The uncertainty of movement now affected the arms, and there was great failure of memory. Her symptoms continually deepened, the speech became more and more incoherent, until it was a confused senseless jargon. She now refused food, and finally stayed in bed in a state of perpetual stupor. The tongue was heavily coated, the breath very offensive. There was no elevation of temperature, or pains, or local soreness."

I was first called to see Mrs. L. May 8; I found her in bed in a sort of stupor, out of which she was with difficulty wakened at all. Getting her partially aroused, I ordered the nurse to put on a wrapper; then, commanding and leading the patient, succeeded in getting her to the head of the stairs, then down stairs. It was necessary to hold her very forcibly, as every few minutes her knees would seem to give way, and she would 'flop' to the floor. All this time she said nothing other than incoherent protestations. When she finally was in the parlor I upbraided her loudly and severely for her deshabille and general appearance. On asking her if she was not ashamed of herself, she said she was, and that she had better go upstairs and change her clothes. On my acquiescing she stood up, and, taking the hand of her nurse, walked upstairs, dressed herself with assistance, and came down recognizing people, but saying very little. I left her eating her breakfast. Her urine was examined at this time; it was loaded with uric acid and urates, although she had been eating very little, had a specific gravity of 1024, and contained no tube-casts, and not the faintest trace of albumen.

She came to the city May 11. She was now completely

demented, knowing no one and not recognizing in any way her surroundings. The pupils were contracted and immobile. She ate no food except milk, which was forcibly given her at regular intervals. Much of the time she lay in a stupor in bed ; then she would have spells of wandering restlessness ; again distinct maniacal outbreaks accompanied by violence and indecent speech, or sometimes attacks of muttering delirium. The tongue was brown, dry, coated to the last degree. The teeth were loaded with sordes; the breath horribly offensive ; the bowels were obstinately constipated. She was treated with purgatives, quinine, chloral and morphine when excited.

During these days there was general tenderness, so that whenever she was taken hold of roughly she would scream out, even rousing from a stupor. There was also on movement distinct pain not located in the joints, but seemingly in the muscles. There were very bad hemorrhoids, and at times the patient lay stupid but moaning, with knees drawn up as though there was abdominal pain. The pulse was quick, never much under a hundred, small and feeble rather than strong. Her mental condition grew worse, she took no note of anything, had to be catheterized, etc.

By May 16, the general tenderness had become very pronounced ; the pupils were dilated and movable ; the intelligence somewhat improved, in that she began to take notice. Salicylates and digitalis were at this time being used freely. On 17th, severe diarrhœa with involuntary passages set in ; also the pulse altered suddenly its character, becoming excessively irregular, from 110 to 150 per minute, with beats of all sizes and rates, and many complete intermissions. General tenderness very marked. Auscultation of the heart showed the first sound very weak over the right base ; at the left apex the first sound was very weak, the second decidedly accentuated. Over the middle cardiac region the sounds were singularly confused, with a peculiar watery and occasionally grating sound " believed to be cardiac friction, but no clear positive to-and-fro friction rale." [Note at the

time.] A blister was applied over the head and one over the heart.

May 18, menstruation had been established ; the pulse had become perfectly regular, 100, and the peculiar middle cardiac sound less distinct. Her mental condition so far improved that she indicated when she desired to pass water, but she could not give a coherent answer to the simplest question.

On the 20th, she, when roused, answered simple questions with some rationality. Severe diarrhœa again manifested itself with involuntary passages and lasted many days, indeed, off and on almost to the end ; remedies simply kept it in check. There were no maniacal outbreaks, and a slow but progressive improvement in her mental condition began very distinctly directly after the violent diarrhœa of the 20th. The first change was in the recognition of her husband ; then, when she had a desire to stool, she would insist on getting out of bed to the commode, although she never said why she got up. Then she resisted food and medicine, clearly because she had a will not to have them. On the morning of the 20th she took food herself, knew where she was, and wondered greatly as to the way in which she had got there, having no memory of past events.

June 23 it was first noticed that something was wrong with the vaginal discharge, but a vaginal examination failed to detect any abnormality. On the morning of June 25 feces were plainly passed by the vagina, and there was discovered a perforation of the recto-vaginal wall very low down, sufficiently large to admit easily the whole forefinger into the rectum. The edges were soft and not well defined. There was no hemorrhage, local swelling, or pain during the formation of this opening, and it was not distinctly sore.

The mental condition of Mrs. L. continued to improve until about the first of June. At the same time her tongue cleaned and her breath became sweet. June 1st, she was entirely rational, recognized her surroundings and friends, servants, etc., in their proper relations. She spoke very

sensibly about her own illness. The memory was very much improved, but by no means normal, and there was an absolute lack of power of mental exertion; but the only thing a casual observer would have noted as peculiar was the character of her voice, which remained very unnatural.

The first indication of a relapse occurred about the first of June in a renewed coating of the tongue; then she began to talk irrationally at night, and her memory rapidly to fail. Then a tendency to drop or elide words from her sentences came on and was very pronounced. Her talk in the night began to be irrational and incoherent. At times she was quiet during the day, at other times very restless. At night she was very wakeful and restless, getting out of bed, wandering about rooms, etc. Much of the time she had an almost uncontrollable desire to pick at her nose and genitals; some days she refused food; at others she took it. The pulse ranged from 100 to 120, and her physical strength distinctly increased.

From this time onward her mental state deteriorated rather than ameliorated. She lost power of knowing those about her, although she still recognized her husband; the memory was entirely lost; in a word, she was in a condition of almost complete dementia. July 18th, the breathing became suddenly accelerated, and some fine sub-crepitant rales were heard posteriorly; on the morning of 18th her breathing grew much worse; the whole upper lobe of right lung was full of very fine crepitation, the left lung also containing rales. This continued for two days, with some slight dulness of percussion on left apex, and then gradually subsided. Some days after this she had an equally sudden attack of œdema of the feet. June 24, she was taken about 11 P. M. with violent dyspnœa, accompanied by fine crepitation anteriorly and superiorly in both lungs, and almost complete absence of breath sounds over the posterior lobes. The dyspnœa steadily increased and she died asphyxiated after twenty-four hours of struggle.

Autopsy.—Kidneys large, plainly in the early stage of

chronic interstitial nephritis. Other abdominal organs normal. Heart somewhat hypertrophied; valves normal. Much excess of fluid in pericardium, but no exuded lymph. Lungs highly œdematous; considerable peritoneal as well as pleural serous effusion.

Brain.—Basilar arteries, walls very much thickened, sufficiently so to distinctly interfere with lumen: smaller arteries also showing signs of similar endarteritis; the upper and to some extent the basal sub-membranous spaces everywhere distended with exuded fluid; fine vessels of the membrane somewhat congested; a very little lymph in some spots in the membranes. Brain substance very anæmic; the convolutions appeared shrunken; consistence normal, no spots of softening or macroscopic changes to be detected; microscopical examination of the convolutions failed to demonstrate anything abnormal, although the cells were perhaps more granular than normal.

There are certain points about this very remarkable case to which it may be worth while to direct attention. The cause of the dementia cannot positively be asserted; but I am very strongly inclined to think that it was, at least in part, due to gouty irritation.

It seems to me well established that gout is capable of causing almost every form of insanity; indeed, insanity is only an increase of the mental conditions frequently seen in lithæmia. Carrol in 1859 said, "Gouty mania is occasionally seen;" and in 1875 Dr. P. Berthier (*Des Nevroses Diathésiques*, Paris) published a collection of 46 cases of nervous disease attributable to gout: one, hallucinations; one, migraine; four, tetanus; three, chorea; one, hypochondria; seven, epilepsy; one, paralysis; and twenty-six of mental affection, including, in these, dementia, melancholia with stupor, mania. Although in some of

these cases the evidence is not at all positive that gout was the *materies morbi*, yet in others the relation seems to have been clearly made out.

In his paper before the International Congress of London (vol. iii. 640), Dr. Raynor supported the following conclusions:—

1. Protracted gouty toxæmia, when not very intense, usually results in sensory hallucinations or melancholia.

2. Sudden and intense toxæmia results in mania or epilepsy.

3. Intense and protracted toxæmia usually results in general paralysis.

4. If there be a tendency to vascular degeneration from plumbism, alcoholism, etc., varying degrees of dementia are produced.

In the discussion which followed the reading of Dr. Raynor's paper, Drs. Savage and Crichton Browne of London both expressed the belief that gout does cause insanity, the latter, however, qualifying by the statement—only where there is hereditary predisposition to insanity.

Further proof of the connection between gout and insanity may be found in the Paris Thesis of M. Belliard (1882, No. 269), in which are detailed various cases.

The facts—that in Mrs. L. the attack was at the time when an explosion of gout was to be expected, that in all her previous attacks mental depression was a distinct feature, that her urine was loaded with lithates, although she was taking very little food, and that there was widespread exquisite tenderness and soreness to movement with febrile reaction—appear to

establish a gouty etiology. The contraction of the lumen of the basilar arteries was seemingly sufficient to check the freedom of blood supply to the brain. Brain anæmia certainly existed, as was proven by the autopsy, and no doubt it aided in causing mental weakness.

It is certainly worthy of remark, as confirmatory of the generalizations made by Dr. Raynor, that the type of mental disturbance exactly corresponded with his conclusions. There was a pronounced tendency to vascular degenerations, and the mental disturbance partook of the nature of dementia.

Passing from the discussion of the etiology of the case, we note the rapid formation of a recto-vaginal circular ulcer as most extraordinary; its occurrence was entirely spontaneous. It seems to resemble in its nervous pathology the rapid eschars which sometimes form upon the buttock and extremities in severe myelitis, or perhaps even more closely the so-called perforating ulcer of the foot.

The temperature sheet of this patient was remarkable, owing to the differences between the two axillas, although, as the autopsy showed, there was no focal brain lesion to account for such difference.

Irregularities of local temperature are well known to occur in focal diseases of the brain, and the temperature in the two axillas in the present case was first tested for diagnostic purposes. The result shows that we may have such irregularities of temperature when there is no local lesion. In a case now under my care at the Philadelphia Hospital believed to be suffering from an acute myelitis, the temperature for many days varied in the two axillas from 0.2° F.

to 1.5° F. To my mind it is evident that we need careful bilateral studies of temperatures in various diseases.

The point to which I want to direct especial attention, however, is that the urine was examined at various times by Dr. Tomlinson without his finding any evidence of contracted kidney, although such lesion existed. My first glance at the patient made me think that she had chronic Bright's disease, but a very careful examination of the chemical reactions, the specific gravity, and the microscopic deposits of the urine so entirely failed to justify any suspicion, that I was entirely misled in this feature of the case. I should here state that my own examinations of the renal secretion were so entirely in accord with the statement of Dr. Tomlinson, that they were not, as they ought to have been, repeated upon various specimens of the urine. As already stated, the aspect of Mrs. L. suggested the existence of chronically contracted kidney, but there was no increased arterial tension, the heart's action whilst she was under my care being uniformly feeble. It is many years since I ceased putting confidence in the absence of albumen as being of much value in disproving the existence of contracted kidney, but I have hitherto believed that reliance could be placed upon the specific gravity of the urine. The importance of examining the specific gravity of the urine cannot be overestimated; and the import of a persistent specific gravity of 1010 or under can scarcely be mistaken; but in addition to the case just detailed the following is of great interest as indicating that normal urine may accompany a fatally diseased kidney.

Mrs. ——, aged 58, the mother of two healthy children, consulted me in the month of April, 1883, on account of certain spells which afflicted her. The history she and her daughter gave was in brief as follows: the attacks first began in 1876 about the time she ceased menstruating, and had continued ever since; they always came on when the stomach was empty, and were sure to happen if at any time during the day she was more than three hours without food; also, if she does not get breakfast upon rising, she is sure to have a seizure. Excessive fatigue increases the tendency to attacks. The spell commences with extreme pallor of face and dark rings under her eyes: if walking, her gait becomes very slow; if talking, her speech slackens and then ceases; she looks around in a dazed, bewildered manner, but does not fall, and is not convulsed at all; she does not become completely unconscious, but does not know where she is or what is going on about her; if a mouthful or two of food be forced down her she arouses immediately, but has no memory of what has occurred during the spell. A careful examination of Mrs. —— resulted in complete negations as far as organic disease was concerned. The urine was normal; there was no failure of mental power, choked disk, palsy, headache, or other local symptom discoverable, and I finally settled down to the diagnosis of gastric vertigo. Under appropriate treatment the patient improved, and I saw her at my office for the last time June 4, 1883.

In April, 1884, I was hastily summoned to her bedside, and found her comatose, with a history of distinct convulsions which were said to have been diagnosed as hysterical, by a neighboring practitioner, who, I was also told after examination of the urine, had stated positively there was no disease of the kidneys. On post-mortem examination the brain was found normal, but the kidneys presented the gross appearances of advanced contracted kidney; and careful microscopical examination by Dr. G. A. Piersoll proved that the condition of the organ was as it appeared.

A second case bearing upon the matter in hand is that of—

Mrs. I. W. T., a large, stout woman who came to my office early last March on account of failing eyesight. Her appearance and description of her symptoms led me to think that she had albuminuric retinitis, but without any examination I sent her to Dr. Harlan, who reported that she had unmistakable albuminuric retinitis, and that no local treatment would be of service.

Her urine had, the day of her return to me, a specific gravity of 1020, and with the nitric acid gravity test yielded no cloud, or one so faint that I could not be sure that it existed, and noted, "believed to have a trace of albumen." According to her estimate, she was passing two and one-quarter pints a day.

April 3d. She passed three and a half pints, having a specific gravity of 1010, and totally free from albumen.

21st. Urine three pints, specific gravity 1015; no albumen.

The symptom of increased arterial pressure and cardiac hypertrophy upon the diagnostic value of which stress has been laid, afforded in the group of cases here narrated no aid. The patients were all large, stout, middle-aged, married women, with full busts, making the recognition of a slight degree of hypertrophy exceedingly difficult, and the circulation in the two more serious cases was certainly enfeebled.

The practical conclusion to be drawn from these case is, that reliance cannot be placed upon a single examination of the urine, but that in any doubtful case of chronic disease it is our duty to examine the renal secretion repeatedly, noting whether albumen appears after a heavy meal of flesh, and whether the urine of abstinence is of abnormally low specific gravity. I

have seen patients who certainly did not have Bright's disease, but in whom an irritant drug or an alcoholic excess would produce albuminuria. It is to my mind very probable that such people will eventually develop renal disease. At any rate, these cases have suggested to me that possibly, as we employ purgatives to make a so-called therapeutic test in a case of suspected typhoid fever, so we might use cantharides, turpentine, or other irritant drug in a case of suspected Bright's disease. If on trial it should be found that a slight irritation would seriously affect the urine, the case should be looked upon with the greatest suspicion.

REPORT OF A CASE OF OCCLUSION OF THE VENA CAVA SUPERIOR, AND OF A CASE OF HEART TUMOR.

By

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PHYSICIAN TO THE PENNSYLVANIA HOSPITAL.

[Read December 3, 1884.]

A MAN seventy-two years of age was admitted into the ward of the Pennsylvania Hospital Sept. 18, 1883, and gave the following history: For thirty five years he worked as an iron-moulder, but during the past year he has been gaining his living as a peddler. He was always very strong and well until during the last two years, when he has had some pain in the lumbar region which has increased in the last three weeks, and during that time he has not slept well. The day of his admission he was standing by the edge of the river looking at some object upon the water, when suddenly and for the first time in his life he was seized with vertigo, and fell into the water. When brought to the hospital the following was noticed as to his appearance and condition: He is tall and spare, having lost much flesh in the last few years, and stoops moderately. The skin is slightly yellow and the tongue furred. The radial arteries are markedly stiffened. The veins on the right side of the abdomen and chest are enlarged, the largest being half an inch in diameter; those upon the left are comparatively slightly enlarged. Prominent veins extend also toward the right axilla. He cannot say how long these veins have been enlarged, as he never gave them any thought, but is sure that they were not

always so. Examination of the heart reveals a murmur which precedes the impulse and the carotid beat. There is slight visible impulse in the epigastrium. At the base of the heart, the sounds are almost inaudible, but they can be heard beneath the sternum; no accentuation of the second sound can be distinguished at the base, although at the apex the second sound is loudly accentuated. The hepatic dulness in the nipple line begins at the sixth rib, and extends slightly below the costal arch. The radial pulse is irritable, but the beat is not increased when the arm is elevated.

September 21. It was noted, the cardiac impulse is in the fifth interspace two inches to the left of the sternum and is rather feeble; there is somewhat more forcible impulse beneath the lower end of the sternum and some visible throbbing of the abdominal aorta. Percussion one inch to the left of the sternum shows the cardiac dulness to begin in the third interspace, and there is flatness at the fourth rib level. Transverse percussion at the fourth rib level shows dulness, beginning at midsternum and extending to the left about four inches. There is distinct impulse in the second interspace to the left of the sternum, and this appears to precede the apex impulse; there is also impulse in the second right interspace, which also precedes the apex beat. There is dulness on percussion in the first interspace to the right of the sternum and in the third interspace. The percussion resonance at the corresponding areas on the left side is natural. There seems to be impaired resonance upon percussion over the upper part of the sternum. The presystotic murmur at the apex is distinct. At the pulmonary area the sounds are very faint but distinct, and the same is the case at the aortic area. At the ensiform cartilage both sounds are distinct, but the first is rather thumping, and is accompanied by a faint blowing murmur which is synchronous with the beat of the carotid artery. There is no undue pulsation of the arteries of the neck, and the veins are full, but do not pulsate. There is a systolic murmur audible in the carotids which is increased by pressure; this is also dis-

tinct in the subclavians. There is marked systolic murmur in the femorals and in the abdominal aorta as low down as the umbilicus. Lung examination shows slightly impaired percussion resonance at the right apex, while the note is full at the left. The respiratory sounds are slightly more feeble at the right than at the left apex. Posteriorly the percussion note is short, but not markedly dull. The respiratory sounds are unusually feeble.

23*d*. The radial pulse is delayed. Pressure upon the enlarged veins at the lower third of the sternum causes the portion of them below the spot pressed upon to become less prominent and full, and produces a sensation of fulness over the eyes. The veins in the right iliac region tend to be enlarged.

28*th*. When the patient is in the erect position, the veins on the left side of the chest between the nipple and the sternum are enlarged. The collection of veins is somewhat cone-shaped, and extends from the umbilicus to the ensiform cartilage, the base being at the umbilicus, which it partly surrounds. The veins going toward the axillæ and those upon the upper part of the sternum are also enlarged. Measurement of the chest on a level with the nipple shows a circumference of thirty-three and five-eighths inches. During ordinary quiet breathing the chest expansion is only one-eighth of an inch, and the extreme expansion on forced inspiration is only one inch. Percussion gives a sound almost dull over both lungs anteriorly and over the upper part of the left lung posteriorly; over the lower portions the note is fuller, but still very short. Anteriorly the respiratory murmur is exceedingly feeble on both sides, almost wanting on the left. Posteriorly the respiratory sounds are very feeble, more so above than below. The treatment consisted at various times of iron, iodide of potassium, and strychnia. During the whole time that he was in the hospital he complained bitterly of vertigo, and finally became very much disheartened, and on the morning of December 12th cut his throat, severing the left external jugular vein. After he

had lost a great deal of blood, the vein was ligated and he lingered until December 20th, when he died.

The clinical history was taken by Dr. Charles Baum, and I am indebted to Dr. Da Costa for the opportunity of continuing the history of the case after I gave up the charge of the ward. The post-mortem examination was made and the note of it furnished by my colleague Dr. Longstreth, who is pathologist to the hospital. It was the Resident Physician, Dr. W. J. Taylor, who first drew attention to the facts that pressure upon the enlarged external veins caused a sensation of fulness in the head and suffusion of the eyes, and that the blood current was downwards in these veins, and it was from these symptoms principally that I was enabled to make the diagnosis.

FIG. 1.

Autopsy.—The first step in the examination was to inject the venous system. An opening was made on the right side of the neck, and the large vein was raised up in the middle cervical portion. The injection-matter used was gelatine colored with carmine. The mass flowed with comparative ease, and was very soon seen flowing and distending the superficial enlarged venous branches over the front of the thorax, at its lateral parts, and at and around the umbilicus.

Allowing time for the warm mass to harden, the examination was continued by dividing the ribs and clavicles in the line of the nipples, and separating the whole mass of the anterior abdominal muscles from the skin. Raising up this anterior mask of the thorax and abdomen, the abdominal aorta and the inferior vena cava were dissected free; the stomach, liver, and diaphragm were freed from their posterior attachments. In the neck, all the vessels and muscles, together with the trachea and œsophagus, were raised up. In the thorax the conjoined organs were cut free from the vertebræ. Thus, the whole mass of organs from the thorax and abdomen were removed *en bloc*, and their dissection was proceeded with from the posterior aspect.

In the left pleural cavity, placed superficially just beneath the membrane, was seen a large venous trunk about one-half an inch in diameter, situated on the bodies of vertebræ and extending from the arch of the aorta to the attachment of the diaphragm below. This vessel was the left azygos; it was on the left side of the aorta. In dividing the tissue of the posterior mediastinum, the veins of communication between the spinal and intercostal vessels were found unusually large, from $\frac{1}{3}$ to $\frac{1}{2}$ inch in diameter, and were thus in condition to carry a large amount of blood from the upper part of the trunk and the head downwards to be emptied into the abdominal vena cava. On removing the organs, the injecting material was found to have filled the veins to the arms and neck on each side, the spinal and intercostal vessels in the thoracic portion, numerous large veins in the dia-

phragm, converging directly towards the inferior vena cava, and also the abdominal portion of that vessel.

In dissecting the neck, numerous large veins (several one-quarter inch in diameter), were found passing behind the trachea, and they served as a communication from the right to the left side. They commenced at about the level of the episternal notch and descended into the thorax passing towards the left, and terminated in the beginning portion of the left innominate or transverse vein. The left innominate vein at this part was very much enlarged, and as the vessel advanced towards the right side to join the vena cava, it was found to be converted into a flattened fibrous cord, and this portion was pretty tightly adherent to the surrounding tissues and the branches from the arch of the aorta lying in contact with it.

A probe passed downwards within the right jugular was stopped by an obstruction at a point where the right innominate vein joined the fibrous cord in which the left innominate terminated. From the posterior wall of the right innominate, the probe entered the mouth of a moderate sized vessel which passed downwards into the thorax; its termination and communications could not be traced; it was supposed to communicate with the right azygos vein, but this vessel could not be found.

On removing the heart the pericardium was found normal. The heart was large; its weight was not taken, as a considerable portion of its auricles were left in connection with the specimen. Both the right and left ventricular walls were thicker than normal.

The mitral orifice was narrowed at its auricular aspect, but its outlet in the ventricle was of normal size. The anterior segment was rendered rigid by the deposit in it of calcareous matter. This deposit extended from near the free border, centrally through the leaflet, and terminated within one of the cusps of the aortic valve. The mitral orifice was thus not only partially obstructed, but the auricular aspect of the leaflet was roughened by the calcareous nodular deposit.

The aortic cusps were all three materially affected ; one was rendered rigid through one-half its depth by the calcareous deposit extending from the mitral valve, another cusp showed a slightly projecting ridge of deposit on its ventricular aspect along the line of junction with the opposing cusp, of firmly organized fibrous material, while the third cusp was thickened, less pliable than normal, but smooth.

The other orifices and valves showed nothing especial to note.

The opening of the inferior vena cava appeared larger than normal, perhaps half as large again. The opening of the superior vena cava was found obliterated. A very fine probe could be forced in about an inch, by tearing tissues at the normal site of the cava. It was found, therefore, that the obstruction existing above in the innominate vessels was complete and extended to the auricle.

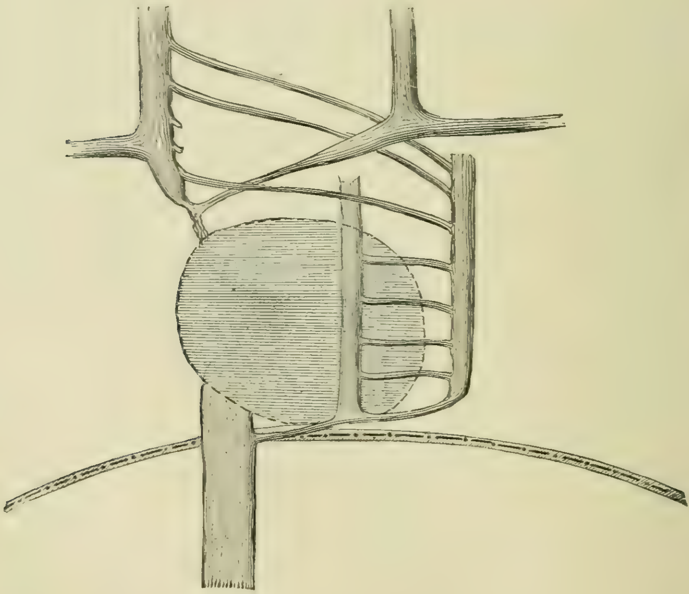
The aorta and its principal branches were very much diseased. Commencing at the aortic orifice, the whole vessel was somewhat dilated, though not aneurismal, and this condition extended to the abdominal portion. Its walls were thicker than normal and very rigid. In most parts of the tube atheromatous changes with calcareous degeneration had taken place, so that in squeezing the vessel between the fingers, the calcareous deposits cracked under the pressure with a gritty sound.

The cause of the compression and final obliteration of the calibre of the superior vena cava with its branches seemed to be due to the rigidity of the aorta together with condensation of the surrounding connective tissues, probably of inflammatory origin. All the connective tissue around the arch of the aorta and first portion of its principal branches was increased in amount and very dense, and firmly adherent to the under surface of the sternum as well as posteriorly to the bodies of the vertebræ. It was in this tissue that the obliterated venous trunks were found, and to which the

tissue adhered so firmly that it was difficult to separate the two in dissection.

The venous blood from the head, upper extremities, etc., found its way to the heart by means of the greatly enlarged veins lying on and around the spinal column posteriorly, particularly the left azygos, as well as anteriorly through the enlarged internal mammary vessels, and the superficial thoracic and abdominal branches. Most of the blood evidently came downwards to the level of the diaphragm and thence found its way directly into the inferior vena cava. Other portions of the blood descended from the superficial portions of the thorax into the abdominal vessels and thence by the lumbar branches to the abdominal vena cava.

FIG. 2.



Rough diagram showing collateral circulation.

The examination of the brain showed a notable fulness of the vessels of both the external and the internal membranes.

Small patches of opacity were found on the arachnoid in some places, along the line of the sulci over the convexity. Some of the convolutions showed in places slight degrees of atrophy, and the furrows between them were widened, and at these parts there had accumulated a small amount of clear subarachnoid fluid.

The brain substance showed no especial alterations. The vessels of the circle of Willis presented themselves as India-rubber-like tubes, patulous and rigidly elastic. No calcareous degeneration of their tunics was found. The same conditions were visible in the beginning portions of the principal branches leading from the circle.

This case was a rare one, and of more than usual interest for several reasons. In the first place, there were unusual opportunities for studying the conditions, both *ante* and *post mortem*, and the physical signs were such that the diagnosis was made during life. The enlargement of the veins of the surface led to the suspicion that one of the cavæ was occluded, and it was decided that it must be the cava superior, because when the large external veins were emptied for the space of an inch or two with two fingers, the blood evidently flowed much more rapidly and forcibly downward when the finger nearest the head was first removed than it did upward when the lower finger was first removed, and this was the case as much when the patient lay in bed as when he stood, thus showing that the current in the enlarged external veins was downwards. Further reason for thinking that the return circulation from the head found its way back to the heart partly by the external veins, was that if they were compressed there was at once produced visible congestion of the veins of the face and suffusion of the eyes and a sensation of fulness in the

head. The presystolic murmur in the heart was very marked and unmistakable, and at all times present while the patient was under observation, and abundant cause for its presence was found in the calcareous deposit with stiffening and roughness of the anterior flap of the mitral valve. The systolic murmur at the base of the heart and in the arteries was caused by the stiffening and roughness of the aortic valve flaps and of the thoracic and abdominal aorta. It was suspected during life that the arteries of the brain would be found to be stiff, and this was the case; still it is very likely that the vertigo with which the man suffered, and of which he complained more than of any other symptom, may have been partly due to the obstruction to the return circulation from the brain caused by the blood having to find its way through much smaller and more tortuous routes than the natural ones, and this view would seem to receive further confirmation from the fact that there was found some flattening and shrinking of the brain convolutions. The cause of the dulness on percussion over the upper part of the right side of the chest anteriorly and the upper part of the sternum, which was so distinctly made out, was not understood during life, but the post mortem showed abundant cause for it in the very great amount of hard fibrous tissue around the ascending aorta and veins in that region, and here apparently was the seat of origin of the whole pathological process. It seems most likely that the changes which finally caused a complete occlusion of the whole of the vena cava superior and the larger portion of both the innominate veins had its origin in a periarteritis, which must have begun

around the ascending or transverse portion of the aorta. This must have proceeded gradually, with a constant increase and condensation of the tissues around the aorta and veins, until at last there was produced the large amount of hard fibrous tissue which the post-mortem examination showed to exist in that region.

A man, twenty-six years of age, was admitted to the ward July 21, 1880. No history of previous disease was obtained, except that he had had frequent attacks of ague, and eighteen months ago had typhoid fever. One year ago he had a chancre, but it was followed by no secondary symptoms. In March (four months ago) he was upset from a boat, and after being two hours in the water was taken out unconscious. Since that time he has noticed that he has had more or less shortness of breath, but felt fairly well and was able to be about until one week ago, when his left arm became paralyzed, and the next day the leg became weak so that he walked with difficulty. Upon admission he was suffering very much from dyspnœa, and his breathing was very rapid, being forty-eight per minute. The pulse was frequent and weak. In walking he dragged the left leg, and the arm on the same side hung powerless, and was much swollen and œdematous, both feet and legs being also œdematous. The face was drawn to the right, the muscles of the left side being flaccid, but the tongue was protruded without deflection to either side. The speech was rather mumbling. The urine was normal.

July 22. A more careful examination of the heart revealed a faint mitral systolic murmur, and that the heart was hypertrophied. There were numerous coarse moist rales audible over the lungs, and there was evidence of some pleuritic effusion.

23*d*. Condition much the same, but he is more stupid and has frequent attacks of dyspnœa.

25th. He is unconscious, and passes his urine in bed. The heart murmur is more distinct.

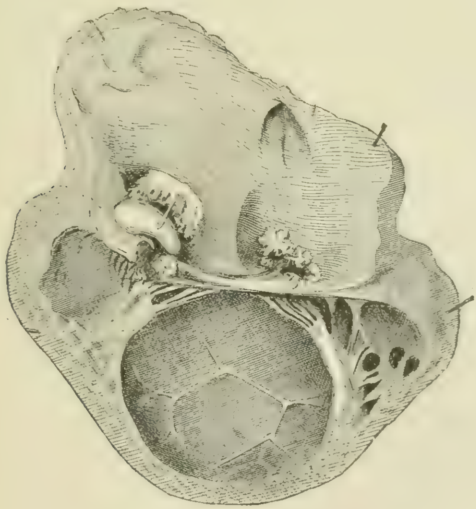
26th. Is sinking, and died July 27th.

Post-mortem examination showed the pleural sacs to contain slightly over a quart of clear serum and that the heart was much enlarged. The fluid and enlarged heart occupied so much space in the thorax that the lungs were much compressed; they felt firm to the finger, but were crepitant throughout, although they were not more than half the natural size. The heart was both dilated and hypertrophied. The right side showed nothing of interest, the valves being normal. The left ventricle and auricle were both much dilated and hypertrophied; this condition was very marked in the auricle. The mitral opening was narrowed, the valve flaps being stretched across it. When looked at from the auricle there was seen a tumor springing from the posterior flap of the valve and projecting into the auricular cavity, which was about three-quarters of an inch to an inch in diameter, and about half to three-quarters of an inch in height. This mass was made up of two different layers, the lower smooth and rounded and the upper rough and uneven, and there were numerous little shreds like rootlets hanging from its surface. There was also on the anterior flap of the valve a spot which was much roughened, and had minute vegetations attached to it. This was probably due to ulceration from friction against the tumor on the other cusp. The aorta was narrow and the valve flaps much thickened, so that the valve was incompetent. The liver was about natural in size, but the capsule was much thickened. The spleen also was natural in size, but rather firm and its capsule thickened. The kidneys appeared natural. The brain presented considerable softening of its right side; this was of the lower parietal lobule and the upper adjoining part of the temporo-sphenoidal lobe; there did not appear to be any involvement of the motor tract. Upon examination of the middle cerebral artery it was found to divide primarily into three branches. The first of these was patulous, but the two posterior ones were blocked with emboli.

Dr. Simes sent me the following report of the results of an examination of the tumor:—

"I have made a microscopical examination of the growth and find it to consist histologically of cells, fibrous tissue, and granular débris. The cells are such as are found in embryonal connective tissue, and appear to be more especially confined to the peripheral portion of the tumor. The fibrous tissue is of the ordinary kind, consisting of delicate fibrils and cells. The granular débris is apparently a metamorphosis of the cells and fibres. I was unable to see any vascular structures. These histological characters, in connection with the macroscopical appearances and clinical history of the case, would lead one to consider the new formation as syphilitic in nature—a gumma."

FIG. 3.



The conditions presented by this patient when he came to the hospital and his history at once suggested that the cerebral symptoms were due to embolus. The distinct murmur heard at the apex of the

heart, together with the signs of hypertrophy and the general symptoms pointing toward rapidly increasing heart failure, were abundantly sufficient to warrant the belief that a fragment of vegetation had been washed from the left side of the heart into some one of the cranial bloodvessels. It struck me also that the origin of the heart disease might be syphilitic. To this suspicion I was led by the history, and because only the year before I had seen a case with a somewhat analogous condition which I reported to the College.[1] The two cases, although in some respects parallel, presented points of marked difference, both in the symptoms presented, in the histories, and in the different modes of death. In the first case there could be obtained no history of syphilis, either initial lesion or secondaries, but the man had been out of health for two years; there was, however, advanced disease of the kidney, and uræmic convulsion had occurred; the man finally died with a very slow pulse and in a certain sense rather suddenly. In the case reported to-night, the history of chancre occurring a year before death was clear, but, as in the first case, no secondary signs had appeared; the man thought himself well until about four months before his death, which was at last caused by the cerebral embolus, and he died with the usual rapid and irregular pulse of mitral disease.

[1] Transactions of the College of Physicians of Philadelphia, 3d series, vol. v., 1881.

SOME CASES OF DISTURBANCE OF THE NORMAL VASO-MOTOR TONUS.

By

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[Read December 3, 1884.]

My object in this paper is to relate some examples of general anasarca due to vaso-motor lesion, and also some instances of acute pulmonary congestion and œdema due to the same cause. The cases given in detail have been selected because in each instance an autopsy was made.

In December, 1879, I reported before the Philadelphia County Medical Society some cases of general anasarca in subjects of malarial poisoning without lesion of the heart, kidneys, liver, or blood. The absence of anæmia or special corpuscular changes in the blood, was established by the use of the hæmacytometer. The dropsy was attributed to depression of the vaso-motor tonus. The line of argument on which the diagnosis was established was by exclusion of all other causes of dropsy, and by an analysis of the experimental evidence bearing upon this topic.

Quoting from that paper: Experiments bearing on this subject were made in 1680 by Lower, who tied the venæ cava and found that œdema followed in the

lower extremities, which he ascribed to the diminished absorption of intercellular fluid owing to venous congestion. Ranvier has proved that œdema depends not only upon diminished absorption, but also upon increased exudation from the vessels. To prove this he first tied the venæ cavæ in the abdomen of the dog, and found that œdema did not appear, for although there was undoubtedly repletion of the arterial system, yet the lymphatics were able to absorb the exudation without any assistance from the veins, and therefore no accumulation of fluid took place.

But if the sciatic nerve was cut upon on one side, intense œdema occurred in the corresponding limb. Venous congestion was undoubtedly present in both legs, for the venæ cavæ had been ligated, but in one the nervous influence proceeding to the arteries through the sciatic nerve kept them contracted, and prevented the exudation of more fluid than the lymphatics could absorb.

In the leg in which the nerve had been paralyzed by division, the vessels dilated, the limb became rosy and warm, and so much fluid was poured out that the lymphatics alone could not absorb it without the aid of the veins.

Ranvier further proved that this was due to the paralysis, not of motor, but of vaso-motor nerve-fibres which are contained in the sciatic. Because, after cutting in different animals motor and vaso-motor nerves in the lumbar region, before they had united to form the sciatic nerve trunk, when the motor fibres were divided as they issue from the lumbar vertebræ before uniting with the sympathetic fibres, complete paralysis of the legs was produced, and no œdema

occurred; but if, on the other hand, he divided the sympathetic fibres passing to the sacral plexus there was no motor paralysis, and œdema occurred.

We can therefore conclude that paralysis of the vaso-motor nerves is sometimes an important factor in the production of anasarca. But the production of anasarca in the instances already mentioned (viz., those in which there is no mechanical explanation or lesion of the blood tissue) is probably also due in part to suspension of the functions of the skin. The function of the skin is not alone dependent on the blood-supply of the skin, but is also under the control of the innervation of the nerves supplying the cutaneous glands.

Dr. Foster states that the skin of dogs and cats can be made to act and sweating produced by stimulation of the sciatic nerve after clamping the aorta, and the same result he has obtained in the leg of the frog by stimulating the sciatic nerve after amputating the leg. The existence of secretory nerves and their special influence over the secretions of the skin, has also been affirmatively investigated by Dr. Isaac Ott and G. B. Wood Field, in a series of original experiments. Their article is entitled Sweat Centres, the effect of Muscarin and Atropin on them.

To sum up: in the anasarca following malaria, under the conditions already stated, we can as readily understand the action of the malarial poison upon the vaso-motor and spinal centres, as we can trace the impaired nutrition of those suffering from repeated malarial seizures to a condition of the nervous system universally credited to the unfavorable action of the malarial poison upon it.

Since 1879 a number of cases of general anasarca have come under my notice, which I have ascribed to sudden depression of vaso-motor tonus (usually through malarial influences), but I have not had an opportunity of making an autopsy on any one of them. The following case of acute general anasarca, however, occurred under my observation in Dr. R. G. Curtin's ward in the Philadelphia Hospital, and through his courtesy I have the privilege of reporting it.

Case I.—A well-nourished man, æt. 38, was admitted to the hospital Oct. 15. Prior to Oct. 8th he had been a healthy man, a driver on the street car by avocation. He had always been used to drinking spirits freely while on duty, to counteract the effects of exposure, but he had not been often drunk. However, after an unusual indulgence he was seized suddenly with œdema of the genitals which, within twenty-four hours, was followed by anasarca affecting the cellular tissues of the abdomen and loins, and finally the limbs became swollen. Associated with the œdema was rapid action of the heart with shortness of breath. On the day of his examination in the hospital the tissues already named were very much swollen, and there was the most profound functional disturbance of the circulation. The heart's action was over 220 as counted by both Dr. Curtin and myself. Withal, the facial appearance of the patient was that of robust health, and the cheeks bore a ruddy hue. The systole of the ventricle seemed normal in force, producing a distinct first sound, and a good impulse could be felt. The physical examination of the lungs revealed only the signs of moderate passive congestion; the liver was normal, and the condition of the urine entirely physiological as shown by repeated tests. After treatment for two weeks a marked improvement was secured, the pulse fell to 120, the anasarca nearly disappeared, and entire convalescence was expected. One morning on rising from the bed the patient suddenly gasped for breath, and fell over on

the bed, dead from asystole of the heart. A careful post-mortem revealed slight hypertrophy with dilatation of the heart, which weighed 14 ounces. There was also slight atheroma of the aorta. The lungs were somewhat congested; brain, on macroscopic examination, normal; and the kidneys natural. The liver was slightly cirrhotic, weighing 61 ounces. The above insignificant modifications in the tissues were credited to chronic effects of alcohol, and the case appears to me to illustrate vaso-motor dropsy, and to sustain the view that various influences as well as malarial are capable of producing vaso-motor paresis with anasarca.

When we review the history of cases of sudden pulmonary congestion and œdema due to or associated with vaso-motor weakness, we find the best illustration in acute œdema of the lungs in subjects of chronic alcoholism. In these cases œdema may develop in a few hours, attributable to the effect of alcohol on the vaso-motor system.

Alcoholic pulmonary œdema differs from the secondary hydræmic œdema of Bright's disease, scorbutus, purpura, anæmia, etc.; because alcoholic subjects are not always anæmic. Alcoholic pulmonary œdema may exist independently of organic heart disease, or inflammatory processes in the lungs, such as pneumonia, capillary bronchitis, miliary tuberculosis, etc. The cases I shall presently report represent pulmonary congestion and œdema from vaso-motor paresis analogous to cases of alcoholic pulmonary œdema, and similar to those cases which occur from deficient vascular tonus from pressure on the vagus or the pulmonary plexus.

Similar forms of œdema occur in acute general diseases, such as typhoid, typhus, and scarlet fevers

associated with feeble heart action. Pulmonary œdema from lowered vaso-motor tonus occurs in the aged or feeble, and is associated with catarrhal swelling of the bronchial mucous membrane.

The following cases of pulmonary congestion with œdema, however, occurred suddenly and in young, previously healthy, persons, who were not directly subject to any of the above predisposing causes.

Case II.—Emma E., æt. 38. Admitted to the Phila. Hospital Oct. 9. Had been in good health up to within two or three days previous to admission. On examination she complained of dyspnœa, and fine crepitant rales were heard all over the lower lobe of the left lung. On the 11th the same variety of rales were heard over the lower right lobe, and had disappeared on the left side; no dulness over either side of the chest, and respiratory murmur slightly harsh, but normal. No evidence of disease of heart, kidneys, or blood, nor of inflammatory disease of the lungs. Temperature 98° F. These physical signs continued until the 14th, unchanged except that the rales grew more moist, and affected both lower lobes. On the night of the 14th she was suddenly seized with intense dyspnœa and very quickly became livid and unconscious. Physical examination revealed intense pulmonary congestion and œdema.

The treatment instituted was hypodermic injection of atropia, at first $\frac{1}{100}$ of a gr. and then $\frac{1}{50}$ of a gr., three injections being given; also two injections of $\frac{1}{50}$ gr. strychnia inside of six hours; together with these measures cardiac stimulants, digitalis, alcohol, and ammonia were freely used, with dry and wet cupping. These measures produced great relief, and in the course of twelve hours the patient was easier and conscious. The treatment by atropia and strychnia, hypodermically injected, was continued by Dr. Jenkins t. d. during the 15th, and the cardiac stimulation several days. The congestion almost entirely disappeared, and we cherished

the hope of an entire convalescence, when on the 19th she had a chill, the temperature rose to 103°, and within two days pneumonic consolidation of the right upper lobe ensued, which proved fatal. An autopsy revealed no lesions save the apex pneumonia in the second stage.

Case III.—Died in the Phila. Hospital, and the notes upon the case have been furnished me by Dr. Mary Farnham, resident physician.

A. J., æt. 50, nurse, weight 150. Medium height, well nourished. Habits at times intemperate. For three weeks suffered from slight bronchitis though quite able to attend to work. Oct. 24, 3 A. M. Patient had a severe chill accompanied by intense pain in the chest, left side, back, and limbs. Chill lasted an hour. At 8 A. M. was bathed in perspiration, surface livid, breathing difficult and painful: dulness on both sides, fine moist rales heard over both lungs. No valvular heart disease, but heart beat feeble. Œdema increased from hour to hour, at 2 P. M. free pinky serous exudation began to ooze from the mouth, at 4 P. M. lost consciousness, and at 6 P. M. died cyanosed. On autopsy the only lesion discovered was intense congestion of the lungs.

In both these cases the history of previous alcoholic indulgence was supposed to be the predisposing cause of the vaso-motor lesion, although the patients did not enter the hospital as subjects of alcoholism.

Case IV.—Occurred in the practice of Dr. Whelen. The patient was a respectable married woman who had been confined within two months of date of these symptoms. She was supposed to be in good health and attended to her domestic duties as usual on the day of her death. At 10.30 Dr. W. was called to see her, and found her livid, with intense orthopnœa, unable to lie down, and a physical examination showed extreme pulmonary congestion of both

lungs with numerous rales. The patient expired at 12.30 within three hours of having been taken ill. Autopsy revealed only the signs of pulmonary congestion without a hint as to a primary cause. I incline to think that in this case the neurility of the vaso-motor system had been reduced by lactation.

I pause at this point to observe that it is important to connect with the vaso-motor system instances of pulmonary congestion occurring in elderly persons with feeble hearts, but without sufficiently serious valvular disease, or degeneration of the heart to quite account for the symptoms. The treatment in these latter cases should be rather a general treatment by hygiene and tonics, than by directly treating the heart, which may be only secondarily responsible.

Digitalis is not so useful as a cardiac stimulant, possibly because it affects the heart too positively before the vaso-motor system is sufficiently acted upon. Indeed the vaso-motor effect of digitalis may be absent when the action upon the cardiac muscle is decided. I allude to my observation of this clinical fact, because I am aware that recently it has been asserted that digitalis possesses a predominant action upon the vaso-motor system. I desire, however, to recur again to this subject.

Naturally the vaso-motor derangement in Graves' disease suggests itself in this connection. The disease, as is well known, is characterized by the association of symptoms connected with the heart, thyroid gland, and eyeball.

The clinical fact which I desire to recall at this point is that the phenomena of Graves' disease illustrate the extreme susceptibility of the vaso-motor

system to exciting causes, since the typical features of the disease are markedly increased at the menstrual period or during sudden emotional excitement.

In the Philadelphia Hospital, in the opposite bed to that occupied by Dr. Curtin's patient, already mentioned, was a case of Graves' disease almost convalescent. The subject was so much excited by the unlooked-for death of his fellow patient that almost immediately the entire series of symptoms of thyroid enlargement, exophthalmos, and cardiac palpitation reappeared.

Finally, in the recital of these cases which form the basis of this paper, the object in view is to call attention to the importance of recognizing the vaso-motor agencies operative in disease, and to indicate a plausible explanation of certain cases of anasarca. And, in addition, to present certain cases of pulmonary congestion due to vaso-motor weakness in the absence of the usual causes of the same.

Treatment.—In the treatment of vaso-motor dropsy it must be remembered that absorption of fluid from the tissues, like its exudation into them, is probably greatly controlled by the central nervous system. Dr. Brunton cites the experiments of Goltze and Nasse, in which the former found that when fluid was injected under the skin of the back of a frog it was rapidly absorbed, so long as the brain and spinal cord were uninjured, but when these were destroyed little or no absorption took place.

Physiologically, absorption is under the influence of nerve-centres, therefore stimulation of these centres will increase their physiological functions. Stimulation of a sensory nerve is capable of inducing con-

traction of the entire vaso-motor system, and Nasse has proved that similar irritation will increase absorption.

Strychnia, digitalis, ergot, iron, and zinc are capable of special impression upon the vaso-motor system, and these drugs are the chief agents with which to combat vaso-motor forms of dropsy. Special diuretics may be used as adjuvants in grave cases but never to the exclusion of the former.

In cases of vaso-motor paresis associated with cardiac palpitation, and other phenomena similar to those seen in Graves' disease, the use of the bromides should be condemned. When vaso-motor dropsy is extensive, agents which stimulate the functions of the skin may also be employed, and cardiac stimulants may be indispensable.

In the vaso-motor paresis associated with more or less pulmonary congestion and œdema, signal benefit has resulted from the liberal use of strychnia and atropia, by the mouth or by hypodermic injection; strychnia by its action as a respiratory stimulant aids in thoroughly oxygenating the blood, and thus promotes the efficiency of the circulation.

But it also acts not only on the dominant vaso-motor centre, but also on the vaso-motor centres distributed through the cord. These centres, to quote Lauder Brunton's words, are so feebly developed as not to heed ordinary stimulation, but can be aroused by the use of strychnia to lend their aid to increase the vascular tonus. This truth has also been proven by experiment, for after section of the spinal cord, which of course paralyzes the vaso-motor centres, the

blood pressure can be made to rise by irritation of a sensory nerve.

The combination of atropiæ with strychnia unites the action of two powerful remedies in urgent cases, and together with cupping these measures anticipate the slower action of digitalis.

Lastly, I desire to take this opportunity to observe that in cases of pulmonary congestion with degeneration of the heart, and vaso-motor weakness, with or without valvular disease, the association of strychnia with some pure cardiac stimulant, such as alcohol, is frequently superior to digitalis because this latter drug seems at times to produce an unfavorable effect. This unfavorable effect well established clinically is difficult to explain, except that the stimúlant action upon the heart and pneumogastrics, slowing and steadying the heart, is not associated with corresponding vaso-motor stimulation, and the pulmonary repletion persists. Again, in valvular heart disease the lesion may be so great that two powerful systoles tend to increase pulmonary congestion by forcing the blood in two directions. Thus, the expression that digitalis depresses the heart is sometimes used, and practically such patients are better off without than with this drug.

SOME OBSERVATIONS ON THE USE OF THE HYDROCHLORATE OF COCAINE,

Especially its Hypodermic Use.

By

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[Read December 3, 1884.]

Hydrochlorate of cocaine is a drug evidently of such power that, on reading the effects produced on the eye, I determined to investigate its properties in other respects, with a view of ascertaining whether it might be of use to the physician as well as to the ophthalmologist. I shall first detail some conclusions I have arrived at with reference to its *local* action.

On the *throat* it undoubtedly diminishes the sensibility, and is serviceable in causing the laryngoscope to be better borne. Moreover, it is of use in irritable relaxed throats, and in instances in which there is spasmodic difficulty in swallowing associated with this irritability, or from other causes. In ulcers at the back of the throat, connected with dysphagia, painting the parts two or three times daily affords considerable relief. Only for this to last, the solutions employed must be stronger than those which have been used—not from two to four, but from eight to

twelve per cent. In tubercular laryngitis the action is excellent. Even a four per cent. solution gives hours of relief, in some cases as many as six hours' freedom from the sense of irritation and the difficulty of swallowing; and stronger solutions relieve for a longer time. The result obtained is far more certain and decided than from the local use of morphia. Compared with iodoform, it is probably less permanent, but as good, or better, at the time.

Dr. Jurist, whom I asked to employ the hydrochlorate of cocaine at the throat clinic of the Jefferson Medical College, has favored me with a note, in which he speaks of the remedy being "brilliantly successful" in relieving pain and making deglutition easy in painful diseases of the pharynx and larynx, pre-eminently in tuberculosis and in syphilis.

Using chromic acid and the galvano-cautery frequently, he found that, by first painting the parts with a four per cent. solution, the employment of these agents could be made comparatively painless, and that the efficacy of these, or indeed of all caustic and destructive means, is not interfered with.

In syphilitic ulcerations especially this was tested, and much suffering prevented. Where only four per cent. solutions are employed, the patient may not feel the caustic application to the abraded surface for about twenty to thirty minutes, but after this it becomes painful. All trials should be preceded by careful cleansing of the parts. The local action of the cocaine is also astringent and hemostatic as well as one destroying sensibility. This local action may also be perceived on tongue and gums. "Although facilitating intra-laryngeal medication, it does not prevent

spasms," Dr. Jurist writes me, "and consequently is valuable in intra-laryngeal operations only on acount of its anæsthetic effects.

As regards the local use of cocaine on other portions of the body, I am able to record some observations made in my ward at the Pennsylvania Hospital. In one instance pain in a hollow molar tooth was speedily relieved by inserting a piece of cotton saturated with a four per cent. solution. It may in passing be remarked that cocaine used hypodermically in the same patient failed to mitigate an attack of intestinal pain of colicky kind which had lasted for two days. A case of earache, which seemed to be neuralgic, was at once relieved by instilling a few drops of a four per cent. solution into the meatus; and a similar observation was made by the resident physician in the ward of my colleague, Dr. Hutchinson. As regards facial neuralgias, the results were less decisive than anticipated. Perhaps they would become more so if we were to rub in an oleate of cocaine over the aching nerves, or larger nerve trunks; or to use a hypodermic for its local use where the disordered nerves are superficial and easily reached. In one instance of neuralgia of the face in which the pain shot into the jaws, painting the gums of the upper jaw with a four per cent. solution gave very speedy relief. For the amelioration of painful and irritable affections of the nasal mucous membrane, hydrochlorate of cocaine, in not less than a four per cent. solution, is of use; and I have known applications with caustics made without pain when the membrane, after being well cleansed, had been painted with the solution. Since becoming acquainted with the action of the remedy, I have had no case of

rose cold or hay fever. But it ought to be of service, and I would suggest its employment in these most troublesome affections.

While discussing its local use it may not be inappropriate to refer to the fact that the solutions of the hydrochlorate of cocaine we all employ—Merck's hydrochlorate—contain less of the alkaloid than supposed; a four per cent. solution, for instance, is only of about three per cent. strength. My attention was called to this by Dr. Jurist, whose remarks, speaking of his observations, I append; and while writing these lines I find that Dr. Squibb has just published the same conclusion:—

"The difficulty experienced in obtaining the cocaine hydrochlorate in bulk, while the solutions were always at command, made it seem desirable to study the latter more closely. In conjunction with my friend, Mr. Stedem, a number of examinations were made. Our later investigations included Merck's manufacture in bulk.

"*Experiment I.*—On adding a dilute solution of ammonia to a solution of the cocaine salt, and then agitating with chloroform, the ammonium hydrochlorate could readily be drawn off with a pipette, leaving the cocaine in solution in the chloroform. By carefully evaporating both solutions the ammonium salt was readily obtained in pure crystalline form. On the watch-crystal into which the chloroform solution was poured there was formed a number of *white acicular crystals surrounded by an areola of sticky resinous material*, light-yellow in color, and altogether amorphous in character. The crystals were soluble in hot and cold water; the resinous product in dilute *hydrochloric acid*, but not in water.

"*Experiment II.*—Another portion of the solution was carefully evaporated over a water-bath. The resulting mass was similar in appearance to the first, but was readily soluble in water. The difference in solubility is accounted for by the

acid state of the residue. When it is remembered that cocaine and its salts have heretofore been described as colorless and crystallizable, and that Merck's product is amorphous granular, and of a light straw color, and further, that chemical manipulation separates a resinous mass from the commercial article, the proposition that our present solutions do not contain the full proportion of the active principle appears to be well grounded."

But what has interested me much about the drug, and what, as far as I know, has not been as yet investigated, is its hypodermic employ, elucidating its general action. In the observations I am about to detail, I have been greatly aided by Dr. Ecroyd, the resident physician in my ward at the Pennsylvania Hospital, and by Dr. Woodbury. We have tried the remedy both on the well and the sick, especially in cases of neuralgia and other painful affections, and have arrived at certain definite conclusions. But first let me speak of the dose. We began with one minim of a two, then of a four per cent. solution, only to find them inert. No influence could be detected on pulse, respiration, or temperature; nor was any local anæsthesia produced at the point of injection. Indeed, no decided effects are produced with less than eight minims of a four per cent. solution, or one-third of a grain of the hydrochlorate of cocaine; and half a grain will show these effects even more strikingly. In some instances two-thirds of a grain were used.

As regards the local influence at the point of injection, there is considerable difference whether a superficial or a deep hypodermic be used. A hypodermic thrown into the superficial layers occasions local anæsthesia, so that the part may be pricked with needles without

these being felt. In one case in which we tried one of these superficial injections in a boy of nineteen, a wheel was produced which was quite insensitive, while all around it sensation was preserved, though perhaps slightly reduced. It is evident, therefore, that if it be desirable to use the hypodermic means of producing local insensibility for the removal of small tumors and the like, a superficial injection immediately into or very close to the parts to be removed will have to be practised. These superficial injections do not occasion subsequent inflammation or abscesses. This is equally true of deeper injections into the tissue below the skin; in the manner in which hypodermics are generally given. But the deeper injections do not produce local anæsthesia of the surface.

Now, as regards the *general* effect of hydrochlorate of cocaine hypodermically employed, it has a little, but not very much, influence on *sensation*. Most patients speak of a sense of warmth all over the body, which, beginning at the point of injection, becomes general in from five to ten minutes; it is, however, not of long duration, the arm in which the injection is practised feeling numb or heavy, or, as one expressed it, "funny." In him, half a grain having been thrown into the left forearm, the sensibility of the skin was diminished from the elbow down to the fingers on that side, and two sharp points were not as distinctly as previously distinguished at the tips of the fingers. There was no change of general sensibility in the legs. The mucous membrane of the lips, tongue, and fauces was slightly less sensitive to sharp points; the conjunctiva was less sensitive, the pupils were dilated.

On the whole, then, there is some general reduction of sensibility, though it is not marked, and is transitory. And this observation accords with others in which one-third of a grain was used, where the alteration of sensibility showed even less; and with a few in which two-thirds of a grain were employed. The general sensibility is therefore only slightly altered.

On the *temperature*, the effect is to heighten it somewhat. This is the record taken by Dr. Ecroyd in the case of a well-nourished man suffering from pains in the back and gluteal region, seemingly due to muscular rheumatism:—

One-third of a grain of hydrochlorate of cocaine was used; no local anæsthesia was produced at the point of injection, and there was no influence on the pain.

At 11 A. M. (just before injection), pulse 76; resp. 17; temp. 97.5°.

At 11.25 A. M. (after injection), pulse 70; resp. 16; temp. 98.5°.

At 11.40 A. M. (after injection), pulse 66; resp. 16; temp. 98.5°.

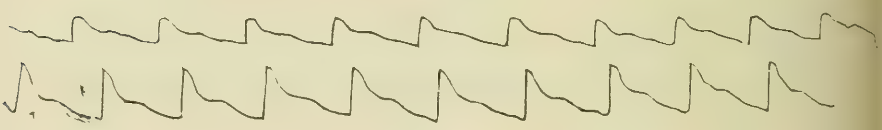
At 11.55 A. M. (after injection), pulse 64; resp. 17; temp. 98.4°.

At 12.10 P. M. (after injection), pulse 68; resp. 16; temp. 99°.

Similar observations were made in other cases; and it may, in general terms, be stated that the temperature rises from half a degree to a degree and a half; that it does not do so abruptly; but that the rise is maintained for several hours. Within ten minutes after the injection has been given, an increased heat is registered. In one case it was four-tenths of a

degree above the figure of starting, and it never reached more than half a degree, which it did one hour and five minutes after the hypodermic injection of one-third of a grain.

The most striking effect of the hypodermic injection of cocaine is on the *circulation.* The pulse may be somewhat accelerated or be slower; but it always becomes fuller and stronger. The frequency of beat was noted to fall from 66 to 54, twenty minutes after the injection. In other instances, there was but little variation; in a few, a slight quickening was detected. But in no instance was there not a fuller and a stronger pulse. We made many observations on these points at intervals of fifteen minutes, the sphygmograph being kept in place to insure uniformity of pressure, and always with the same result. I select from numerous tracings this one,



taken in a case in which half a grain of the hydrochlorate had been given hypodermically; the upper line is the pulse-trace before the injection, the lower fifteen minutes after. The higher vertical line of ascent, the more pointed summit, show the increased force of cardiac contraction; the more sudden fall in the line of descent, and the well-marked dicrotic wave, indicate no increased, rather lessened, arterial tension. And all our observations tend in the same direction. Tracings taken an hour after the

injection still show these characteristics: but the effect gradually passes away. While marked, there is decided increase of the heart's action; the apex beat is more distinct and forcible.

On the *pupils* the influence is very marked. They become speedily dilated; and with the change, uncertainty of vision is complained of. The dilatation of the pupils, following the hypodermic injection, does not last more than a couple of hours, but during this period ophthalmoscopic examination is of course very easy.

On the *secretions*, I have not as yet fully studied the drug. On the bowels it had no influence; the urine appeared increased in quantity, and the specific gravity decidedly lowered, while the phosphates were found to be increased. But our observations were not very numerous or definite on these points.

Summing up now some of the general effects observed, the drug, used hypodermically in the doses mentioned, failed to relieve attacks of intestinal pain, and was useless in instances of obstinate neuralgias, especially in sciatica. This was especially shown in a case at the Pennsylvania Hospital, in which about ten hypodermic injections were given, varying between one-third and half a grain, but in which no decided effect on the pain or the disease was produced. It is, however, fair to state that the case was of a year's standing, and had resisted blisters and chloroform injections. The cocaine influenced somewhat the dull pain; but did no permanent good. Neither in this case nor in others did it induce sleep. In certain very superficial neuralgias, its local action, not its constitutional influence, does

temporary good. As an anæsthetic, its local action is the one which will give it its greatest value; and in diseases of the eye, ear, throat, tongue, nose, and rectum, the insensibility it gives rise to suggests a wide range of application. But this insensibility cannot be produced to a sufficient degree through the constitutional effect of a hypodermic.

Yet thus resorted to, the remedy has other and valuable uses. The effects on the pulse and temperature recorded in these observations, suggest its application in many a condition of collapse, of weak heart, or heart failure; and its employ in low fevers, too, as a cardiac stimulant is a self-evident proposition. How permanent the benefit, how often the doses must be repeated, are matters which experience alone can determine.

A CASE OF ADDISON'S DISEASE.

By

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[Read January 7, 1885.]

H. P., 27 years of age, married, with a healthy family, was born and always lived in the mountains of Pennsylvania. He worked hard, and often, when in the woods hunting or lumbering with men stronger than himself, overtaxed his strength. On one occasion he hurt his back so badly as to lay him up for a week or so. He often wrenched his back in lifting heavy articles. He drank a little whiskey, smoked a little, chewed to excess, and ate his meals rapidly, but for a man in his position his habits were good. He never had syphilis, and was never exposed to such depressing influences as worry and anxiety. There was no inherited family predisposition. He had the ordinary diseases of childhood, but was usually well until the age of twelve years. At that time his stomach began to trouble him, and he noted that indiscretions in diet would cause dyspepsia, with its usual symptoms, which would disappear when the diet was regulated. As he grew older, these symptoms of dyspepsia became more frequent and troublesome, and were often associated with spells of diarrhœa and colicky pains in the abdomen. Between the attacks the bowels were regular.

During the past six years his strength gradually failed, and for a year he had been unable to follow his accustomed avocation. His flesh was well preserved until six months before coming under observation, within which time he had lost ten pounds. For three or four years, he had had a pain between the shoulders, which, however, was not very troublesome. During the past few months, the dys-

pepsia had been almost constant, and more troublesome, and in addition there was a continual distress, and pain across the stomach running through to the back. There never were spells of vomiting with the dyspeptic symptoms, and during all this time the appetite was uniformly good.

Two and a half years ago, he noticed that his color was darker than usual. This he attributed to exposure to the sun, and during the following winter the discoloration grew less distinct, but it returned with the return of spring, and last winter, instead of growing less, the color became more intense.

Upon entering the wards of the Philadelphia Hospital, October 1, 1884, he thought that the color of the cheeks was lighter than it had been six months before. The general condition had failed no more rapidly since the appearance of the discoloration than it did before. His hair had begun to turn gray at the age of twenty.

The notes upon admission state that he was apparently a well-nourished man, with dark brown hair sprinkled with gray: the pupil of the right eye a trifle larger than that of the left (probably physiological). "He takes very little interest in anything but his family, and when away from home he suffers acutely with homesickness. Last spring he came to the hospital to stay, but ran off in twelve hours, stating that he could not stand the place. His mental condition is good, and he sleeps well. The tongue is moist, with only a little yellow coat. After eating he has a feeling of fulness in the stomach, and has a dull pain across the pit of the stomach and through into the back. This is aggravated by the presence of food, and is sometimes sufficiently severe to keep him awake. There is no tenderness, no nausea or vomiting. The bowels are costive, and since being more careful in his habits he does not have spells of diarrhœa as frequently as formerly. He has great weakness and a feeling of excessive muscular debility, and any exertion produces breathlessness, palpitation of the heart, and complete prostration. There are occasionally attacks of palpitation and short breathing even without exertion.

"The whole surface of the body is darker than natural, the discoloration being most marked on the face, where it extends from a little under the roots of the hair down over the neck to the line of

the collar, and on the backs of the hands, extending to above the wrists, where the color is, at least, as dark as the complexion of a mulatto. It, however, has not the yellow tinge of the mulatto, but is more of a mahogany tint, as though stained with walnut-juice. It is not uniform, but is distributed in patches with little intervals of lighter-colored skin. It is not at all affected by pressure, and is not bounded by a sharp line of demarcation, but fades gradually into the surrounding skin. The lighter-colored skin is somewhat darker than the average. There is no swelling. The whole surface of the body is darker than usual, and, as he states that it is darker than it used to be, we may say that it is darker than normal. He states that the color deepens as the surface becomes cold. The finger-nails are of the ordinary tint, and contrast strongly with the dark fingers. The mucous membrane of the inside of the lips is darker than natural, and scattered over it are irregular, sharply defined purplish patches."

An examination of the blood was made by Dr. William E. Hughes, to whom I owe this admirable history. The number of red globules was 5,130,000 to the cubic millimetre. There were 9000 white corpuscles to the centimetre, or 1 to 560 red. The red globules were a trifle smaller and a little paler than normal, and some of them deeply pigmented. The white were normal in size, some containing pigment-granules, and some deeply pigmented. There were also some free pigment-granules. I must add that his temperature was normal, or even below normal, as a rule, though on one or two occasions there was a rise to 99½° or even 101° or 101½° for a few hours, apparently due to gastric irritation.

He was confined to bed. His diet was carefully regulated, and was varied from time to time to suit the failing powers of the stomach. For the most part it consisted of milk. Several applications of the actual cautery (Paquelin's) were made over the renal region posteriorly. Internally, he took aromatic spirits of ammonia, which proved an acceptable and useful stimulant to him. He lost strength steadily and quite rapidly, and from the date of admission, October 1st, to the date of death, November 18th, he lost twenty pounds—from one hundred and twenty-four down to one hundred and four pounds. The pulse grew feeble and rapid. There were from time to time unaccountable sudden failures of appetite and digestive power.

He was very low-spirited, and complaining constantly of distress and of inability to sleep quietly. Vomiting occurred occasionally during the last three weeks of his life. The pulse was barely perceptible at the wrist for two weeks before death. The discoloration grew somewhat darker towards the close. For the last forty-eight hours he was profoundly unconscious.

The post-mortem examination was made five hours after death by Prof. William Osler, assisted by Drs. Formad, Hughes, and Sykes.

Body of medium size, fairly well-built man; moderate wasting, particularly of the face. The skin dark in color, most marked on face, neck, hands, penis, and scrotum. In those regions the color is that of a light mulatto. Rigor mortis not present. Panniculus on abdomen about one-half inch in thickness. Rectus and other abdominal muscles are of a dark-red color. The omentum is fatty, adherent to the parietal peritoneum in right hypochondrium and to front of cæcum. There is extensive perihepatitis and adhesion of the liver to the diaphragm by fibrous bands, which can with slight difficulty be torn through. The surface of rest of parietal peritoneum is a little opaque. Membrane over small intestines is smooth; over mesentery presents numerous superficial fibroid opacities (these are specially seen towards the root of the mesentery). The mesenteric and general subperitoneal fat *considerably increased* in amount. The coils of small intestines are somewhat shrunken. The stomach is also small and atrophic. In the preliminary dissection, the right supra-renal capsule was found firmly attached to the under surface of liver and to the side of inferior cava. No special thickening about its outer surface, but there is a fine capsule of fat. On attempting to separate the kidney and supra-renal capsule from the liver, firm fibrous union is found. The cardiac end of stomach is attached to the enlarged left supra-renal capsule, and the spleen and tail of pancreas are closely united to it. Kidneys, supra-renal capsules, spleen, and retro-peritoneal tissue taken out together for dissection.

Lungs.—Pale; crepitant throughout; a little congested at bases; no pleuritic adhesions; no caseous masses; one or two small pigmented nodules; bronchial glands not enlarged.

Heart.—Loose dark clots in right auricle, projecting into veins;

same in right ventricle; left ventricle in rigor mortis; left auricle contains a dark clot; valves healthy; no atheroma of intima of aorta; muscular substance a little rusty-brown in color; does not look fatty.

On removal of heart, a very considerable amount of blood exuded from the veins.

Liver.—Perfectly normal-looking, good color, and plenty of blood; the bile-vessels are normal.

Stomach is very small; contains only bile-stained fluid; the cardia a little thin; mucous membrane normal.

Kidneys.—Of average size; color good, except immediately beneath enlarged capsules, where the tissue is soft, pale, and many of the tubes are fatty

Intestines.—Cæcum and colon full of firm, hard, scybala; ileum normal; Peyer's glands distinct; two inches from cæcum one long patch begins, which measures six inches in length, and presents in a typical manner the shaven-beard appearance; the solitary glands are a little swollen and prominent. Bladder is full of urine; the mesenteric glands not swollen; the mesentery is very fat.

Spleen.—Slightly enlarged; closely united to diaphragm and left supra-renal by old adhesions; pulp of medium consistency.

Lymph-glands.—Some of the lymph-glands of the abdomen, particularly those of the mesentery and retro-peritoneal, are swollen, grayish-red in color, and several of them contain grayish, translucent nodules, with opaque centres, looking like small tubercles; some as large as peas, and the centre distinctly caseous.

Supra-renal Capsules.—The *right* is closely adherent to the kidney, diaphragm, and inferior cava, and to the liver above. It is two and a half inches in length, one and three-quarter inches in vertical diameter, and rests upon the top of kidney, not descending into the hilus. It is exceedingly hard and firm, and on section shows a peripheral, dense, grayish-white, partially translucent tissue, and centrally three or four yellow, caseous masses, separated from each other by strands of fibrous tissue of almost cartilaginous hardness.

Left Capsule is much larger; covers the top of kidney, and descends on its inner surface well into the hilus, measuring three and a quarter inches in length, two inches vertical, and two and a half inches in thickness. The lower end of the spleen is firmly united to

it, and tail of pancreas is also attached to it. The splenic artery and vein run along its anterior surface in the thick fibroid fatty tissue, and at one point the calibre of these vessels is very materially reduced.

The mass is extraordinarily firm, cuts with the greatest resistance, and presents identical characters with the right, except that at the most anterior region there is a pocket of creamy-looking pus, and at a central spot the caseous matter has undergone softening. At the most anterior part, close to the cœliac axis, the left semilunar ganglion is directly embedded in the fibrous tissue, and can be seen as a rounded grayish mass about one-quarter inch in diameter, and three small nerve-fibres can be seen passing from the ganglia in the dense tissue about it.

Semilunar Ganglia.—The splanchnic nerves look normal, and can be traced directly into the semilunar ganglia; as the right passes on the crus there is a small ganglion developed upon it; it is free from all adhesions; the branches are readily dissected and look normal. The left semilunar ganglion is directly involved in the fibrous tissue of the supra-renal capsule, as stated above: the nerves of the left ganglion, as they pass out, are involved in the cicatricial tissue of the left suprarenal capsule, and cannot be dissected, but can be seen on section; several filaments can be traced to the branches of the cœliac axis.

At the concave margin between the two lunar ganglia there is a small dark-yellow body, probably a supernumerary adrenal. A bunch of normal-looking nerves passes from the right directly to the left semilunar ganglion. The branches passing to the right supra-renal can be traced in the fat and along the artery; they are neither numerous nor large.

Microscopic Examination.

Left Semilunar Ganglion.—Teased portions from the centre of it show innumerable ganglion-cells, most of which are very darkly granular, but the nuclei and, in places, the nerve-processes can be distinctly traced; nerve-fibres, medullated and non-medullated, are

numerous, and in many places show remarkably slight change; in some cords the fibres are less distinct, fat-granules numerous, nuclei elongated, and the process of disintegration appears to be going on. In sections the ganglion-cells can be seen separated from each other by a considerable amount of nucleated tissue. In places they are still closely set together, but in others they are in a great part atrophied, and only one or two can be seen in the fibrous connective tissue, being recognized by the dark granular pigment. The section showed the nerve-fibres through the hard sclerotic tissue, and the chief change noticeable is a marked elongation of the nuclei, and, indeed, a multiplication of them.

Right Semilunar Ganglion.—The cells are very readily isolated; the nuclei distinct, and in each instance surrounded by tolerably dense, dark pigment-granules; the nerve-fibres appear normal.

Several portions of nerve-fibres passing out from the right semilunar ganglion were examined, none of which showed any special change; the cord of the thoracic sympathetic and the ganglia normal; the cells of the latter moderately pigmented.

Marrow from Rib.—It is dark red in color, does not look fatty, and presents the character of ordinary red marrow; many of the colorless cells are very granular and of average size; no great number of small ones; nucleated red blood-corpuscles tolerably abundant; no cells containing red blood-corpuscles.

Marrow of Vertebra.—Rather more fatty, of a deep, rich color, and differs further from the fact that it contains a number of cells containing red blood-corpuscles; in places these are extraordinarily numerous and large.

Heart-muscle of left ventricle in a state of brown atrophy; a few of the fibres present fat-granules throughout them.

Spleen-pulp.—In addition to the ordinary elements, there are also very numerous corpuscles containing red blood-corpuscles in all stages of degeneration.

It will be seen that this case was a very typical one of Addison's disease, both as to its clinical symptoms and as to the lesions present. The absence of anæmia is shown by careful examination of the blood, indicating

that the main cause of the symptoms was to be sought in connection with the sympathetic nervous system; and the extensive and positive lesions carefully made out by Prof. Osler in this case fully confirm this view. Undoubtedly in many cases of Addison's disease the chief cause of the symptoms is to be found in such nervous implications and lesions; but in other instances it is clear that there is a progressive anæmia associated which must play a part in causing the symptoms. In cases where the exudation in the capsules undergoes cheesy degeneration with absorption of the disintegrating organic materials, it is not improbable that there may also be an element of sepsis which would aid in inducing cachexia, even if it did not lead to tuberculosis, which, as is well known, not rarely appears in the late stages of Addison's disease.

The frequent allusion to severe strains or injury of the back in the history of this case suggests the possible origin of the disease of the capsule from repeated irritations caused by violent muscular strain or by sudden shocks to the frame. It will be noted that in a considerable proportion of cases this disease has arisen in subjects liable to such causes as the above.

[After the reading of the preceding paper:—]

Dr. Frederick P. Henry remarked that this was undoubtedly a genuine case of Addison's disease, and remarkably free from the frequent complications of that affection. The rarity of Addison's disease in this country may be judged of by the fact that in the eleven volumes of the *Transactions of the Pathological Society of Philadelphia*, there are reports of only two cases, and these were made by Dr. Henry, in whose practice they occurred. They are to be found in vols. v. and x. The specimens from one of the cases are now in the museum of the Episcopal Hospital. Dr. Hughes's report upon the condition of the blood in this case is very interesting. Most writers upon Addison's disease are accustomed to attribute the profound adynamia, so characteristic of the affection, to a high degree of general anæmia, in spite of the absence of any facts to support such a theory. This adynamia is due to an insufficient supply of blood, although of good quality, to the supra-diaphragmatic portion of the body, the result of an accumulation of blood in the abdominal bloodvessels caused by vaso-motor paralysis. Free pigment in the blood has only been once before observed, so far as the speaker is aware, by Van den Corput (*Gaz. Hebdomadaire*, 24 Juillet, 1863). The changes observed in the sympathetic ganglia are of undoubted interest, but it must not be forgotten that typical cases of Addison's disease have been observed without perceptible lesion of these ganglia. Eulenberg and Guttmann (*Journal of Mental Science*, January, 1879) have collected twenty cases in which changes were found in the nerves of the supra-renal plexus and the ganglia of the solar plexus, opposed to which they place twelve cases in which careful examination demonstrated no change whatever. The positive observations included fatty degeneration (found by Queckett in one of Addison's original cases), swelling and redness of the nerves of the lesser splanchnic and ganglia of the solar plexus, atrophy, pigmentation of the ganglionic cells, increase of connective tissue in the ganglia and in the neurilemma of the nerve-fibres, and caseation of the semilunar ganglia.

Primary affection of the abdominal sympathetic has been appealed to as a cause of numerous other affections, notably of Bright's disease, by Drs. Da Costa and Longstreth, on the basis of careful microscopic examination of the ganglia in nine cases (*Am. Journ. Med. Sci.*, July,

1880). The accuracy of their description of the condition of the ganglia has been confirmed by Dr. Robert Saundby (*Brit. Med. Journ.*, January 13, 1883), whose observation has been still more extensive (fifteen cases), but he entirely dissents from their conclusion as to the primary nature of the sympathetic affection, and points out that similar changes have been found in the ganglia in diffuse eczema, pseudohypertrophic muscular paralysis, pernicious anæmia, glio-sarcoma of the brain, general paralysis of the insane, cholera, and diabetes. He points out, in conclusion, that Giovanni (*Patologia di Simpatica*, Milan, 1876) "found cellular infiltration of the sympathetic ganglia in an immense variety of visceral and general diseases, showing that structural changes in the organs are very generally accompanied by signs of irritation in the ganglia."

It is not necessary, however, that there should be a destructive lesion of the solar plexus and semilunar ganglia in order to produce vaso-motor paralysis of the abdominal vessels. Irritation of a sensory nerve produces vaso-motor paralysis in the irritated region, and the well-known experiments of Goltz (*Klopfversuch*) have shown that irritation of the intestines produces complete vaso-motor paralysis of their bloodvessels, causing thereby so great an accumulation of blood that the animal shows symptoms of syncope, the same as if it had been bled copiously. In Addison's disease, from the beginning of the morbid deposit in the medullary substance of the supra-renal capsule, an irritation is transmitted to the semilunar ganglia and solar plexus, by which means a vaso-motor paralysis of the abdominal vessels is produced, as in the experiments of Goltz. This constant hyperæmia leads to enlargement of the glands of Brunner, the solitary glands, and Peyer's patches, so constantly met with, and, when more intense, to catarrh and ulceration of the intestinal mucous membrane. It also accounts for the dark color of the liver, spleen, kidneys, and pancreas so often observed, as well as for the brown color of the peritoneum sometimes noted, notably in a case of Severini. Indirectly it explains the anæmic and dry condition of other parts of the body, and fully accounts for the symptoms of muscular weakness, syncope, and gastro-intestinal disturbance.

In the case under discussion no mention is made of the size of the heart, which is noted as being small in numerous autopsies, and the seat of fatty degeneration in several. On account of the abdominal hyperæmia, the quantity of blood reaching the heart at each diastole

is less than under normal circumstances, and thus the invariable weakness, smallness, and softness of the pulse are explained. The heart is never normally distended, and thus its cavity becomes permanently diminished. On account of these heart-changes the abnormal distribution of the blood due to the abdominal hyperæmia is still further facilitated. Anæmia of the central nervous organs explains the dizziness, tendency to somnolence, lassitude, and lowness of spirits so often observed; also attacks of prolonged prostration and loss of consciousness, with convulsions; sometimes a condition of collapse that has been compared by Wilks to that of cholera. All the nervous symptoms of Addison's disease are not, however, to be explained by the theory of cerebral anæmia: such, for example, as restlessness, sleeplessness, psychical disturbances, clonic spasms in various muscle-groups, and sensory and motor paralyses. In such cases there is probably irritation of a cerebro-spinal vaso-motor centre, causing local hyperæmias of the central nervous organs. There are two paths by which such a reflex action may travel: 1, through the cœliac axis and its connection with the medulla spinalis; 2, through the branches of the phrenic and pneumogastric distributed to the supra-renal capsules; and the degree of this irritability probably varies in each individual.

In connection with the abnormal deposit in the skin in Addison's disease, certain observations of Thudichum are of great interest. For sixty-five consecutive days, he examined the urine of a patient of Dr. Burdon Sanderson. Without complicating fever or diarrhœa, there was a great diminution in the daily amount of urine excreted; the sp. gr. was 1020 and upwards, and the reaction acid. The observations were chiefly directed towards the determination of the percentage of uromelanin, omicholin, and uropittin, three products of decomposition of urine-pigment, of which the first is the most important. It has almost the same chemical composition as the melanin of the choroid and of the melanotic tumors, and, like the pigment in the skin, is iron-free. Thudichum found these pigments greatly reduced in amount, the uromelanin never rising above one-twelfth the normal. The speaker has been unable to find any record of a spectroscopic examination of the pigment deposited in the skin in Addison's disease, and would suggest the importance of such examination being made. There is an iron-free product of decomposition of hæmoglobin, namely, hæmatoporphyrin, which is iron-free hæmatin, the absorption-bands of

which are figured in Hermann's *Handbuch der Physiologie*, Bd. iv., Theil I. It would be interesting to compare the absorption-bands of these two iron-free pigments, both of which have their origin in the coloring-matter of the blood. It is evident that the diminution in the amount of urine-pigment may be in relation to the excess of pigment in the skin. The observations of Thudichum await further confirmation.

REPORT OF A CASE

OF

FOREIGN BODY IN THE PHARYNX; DEATH RESULTING FROM FAILURE TO RECOGNIZE ITS PRESENCE AND TO REMOVE IT IN TIME.

By

WALTER F. ATLEE, M.D.

[Read January 7, 1885.]

It is not at all an uncommon occurrence to have a visit from a patient who complains of having swallowed something that is still sticking in the throat. In almost every one of these cases there is no foreign body in the passage. Those patients have a local pain, in some cases the result of injury by a hard body hurriedly swallowed, and they are so entirely convinced by this sensation that a foreign body has lodged there that it is impossible to make them believe otherwise. The surgeon himself may make a mistake, and think he feels a something that ought not to be there. I heard even Nélaton say that in a certain case, after pushing his finger deeply into the pharynx and feeling a small resisting body, he made several attempts to seize it with the forceps before discovering it to be the great horn of the hyoid bone.[1]

[1] See Clinical Lectures on Surgery, by M. Nélaton, p. 54.

I made observations somewhat similar to these to a man who came, in great excitement, on the evening of the 30th of last December, to take me to consult with a well-known and experienced physician in the northern part of the city, in the case of a child in a dying condition from the presence in the throat of a pin, as the father protested, but which the doctor had not found and did not believe to be there. On the afternoon of Christmas day, five days before, the father said his child, just seventeen months old, most certainly had a pin in her mouth, that it had disappeared when he went to take it out, and the symptoms of throat-trouble began at that time. For five days the child had taken food with great difficulty and reluctance, keeping the hands in the mouth as if striving to pull something from the throat.

It will here be called to mind that while more bulky objects generally become arrested at the junction of the pharynx with the œsophagus, where the tube is narrowest and least easily expansible, a thin and pointed body, such as a pin, generally sticks between one of the pillars of the fauces and the tonsil, or thereabouts. Again, when such a body stops in the pharynx, that which takes place is owing less to its size than to its shape: it is a body that, as a rule, cannot be pushed further instead of being extracted, as is often done with bodies of another kind. *It must always be extracted.*

On reaching the house, the child was found lying in a cradle, on her left side, the head thrown back, in a state of stupor, from which she could be roused but very imperfectly. She had had during the day several convulsions. The lips were bluish, and the whole countenance extremely pale, with a bluish tinge. There was

a swelling in the neck on the right side, which was the uppermost, below the mastoid process, posterior to the line of the ear. This swelling was not so hard as in cases of diphtheria; it had the feel of cellular tissue affected by acute œdema and not by phlegmonous inflammation. In the mouth was some ropy mucus tinged with blood, but there was no repulsive odor. While examining these appearances it was suggested that the child had the mumps. There was no swelling, however, about the temporo-maxillary articulation, nor anywhere anterior to the ear. It was posterior to the ear and inferior to the mastoid process. Moving the head, the left side of the neck presented a condition similar to that on the right, though not so marked.

The attending physician said his treatment had consisted mainly in the administration of chlorate of potassa in a syrupy solution. He did not believe in there being any foreign body in the child's throat, but was very willing to have search again made for it. When searching for it himself, he had made use of his eyesight only, and had never passed his fingers into the pharynx.

The child was taken up and held in the nurse's lap in a convenient position for the examination of the pharynx. The doing of this roused her somewhat, so that a few drops of chloroform were used to quiet her. The mouth was then opened, and the jaws kept apart by a large cork. When the finger was passed into the throat a pin was encountered, firmly fixed there, and seemingly stuck, one end between the right tonsil and the pillars of the fauces, and the other in the posterior wall of the pharynx. The extremity of the forefinger of the left hand being kept in contact with the pin as a guide, a

dressing forceps was made use of; and on the second attempt to seize it, and with the use of some force and some manœuvring to dislodge it, the pin was withdrawn. The pin was exactly an inch and three-sixteenths in length, and it was bent in the centre at an angle of about a hundred and twenty degrees. This bending could scarcely have been produced by the force used in extracting it from the throat.

As there was, of course, great difficulty, even impossibility, in making the movements of deglutition, and every attempt to swallow must excite reflex movements in the pharynx and retard cure, it was advised that no food or medicine should be given by the mouth. In order to try to nourish and stimulate the patient, appropriate enemata were ordered. The child, however, never revived, the stupor became more and more profound, and she died the following day,—just twenty-four hours after the removal of the pin. The cause of the trouble, the source of the irritation, having been gotten rid of, hopes were entertained that the patient might recover; but, as is often the case in children when the exhaustion and the enfeeblement of the nerve-centres have been so great that repeated convulsions are the result, she never again became conscious, and life gradually went out.

The history of this case teaches nothing new, but it is well at times to be reminded of what may occur, and of the extreme care and watchfulness that are at all times demanded in the practice of our profession, in order to avoid sad and even fatal mistakes.

Nearly a century and a half ago, it was in 1743, Hevin wrote, in the first volume of the *Mémoires de l'Académie royale de Chirurgie*, his *Précis d'observations sur les corps étrangers arrêtés dans l'œsophage.* In this

paper, a hundred and sixty pages in length, by the recital of the facts themselves, by a long series of all the cases that had been until then recorded in ancient and modern times, are set forth the diversity of effects produced in the pharyngo-œsophagean canal by not only bodies of different kinds, but also by those of the same shape and nature, the different success of the means employed, the dangers more or less urgent to which the patients are subjected by the presence of these foreign bodies, and the consequences to be dreaded when they fall into the stomach.

About fifty years ago, Mondière, in the *Archives de Médecine*, wrote a number of "Memoirs,"[1] in which he says that although all the means for fulfilling the various indications that can be called for are all in this paper of Hevin, yet it does not set forth as precisely as should be the dangers that may result in cases of this kind from leaving too much to nature. His object in writing, he adds, will be successfully attained if he succeeds in warning young practitioners against a security to which they cannot always trust without regret, and he gives cases of spasm, convulsions, suffocation, inflammation, scirrhus, abscess, and saccular dilatation of the œsophagus, the result of failure to remove a foreign body arrested in the canal.

It is with this object—namely, that "of warning against a security that cannot be always trusted to without regret"—that I have ventured to take up the time of the College this evening with reading the history of the foregoing case. "*Bene facit qui ex aliorum erroribus sibi exemplum sumit.*"

[1] For Mondière's "Memoirs," see the Archives Générales, vols. xxiv., xxv., xxvii., and xxx. of the first series, and vols. i., ii., and iii. of the second series.

AN APPARATUS DESIGNED TO FACILITATE THE REMOVAL OF FIXED DRESSINGS.

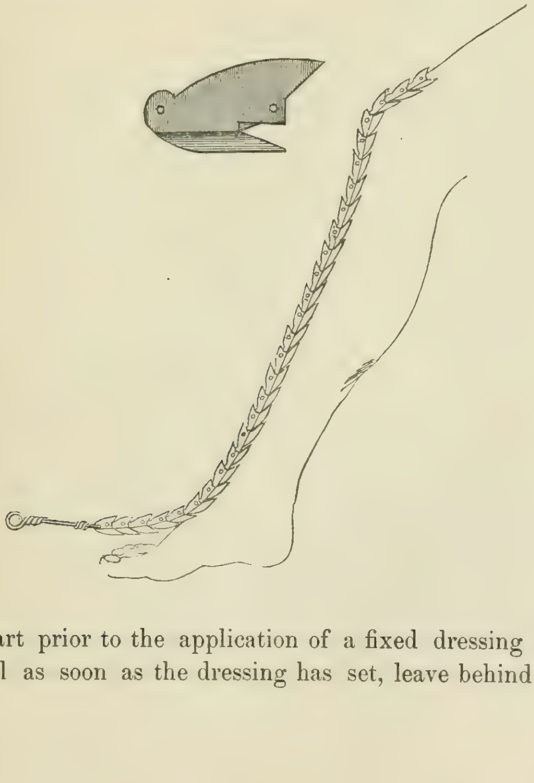
By

WM. BARTON HOPKINS, M.D.,

SURGEON TO THE EPISCOPAL HOSPITAL.

[Read January 7, 1885.]

It consists, as shown in the accompanying cut, of a vertebrated chain constructed of brass, and so formed that when it is placed



upon a part prior to the application of a fixed dressing it will, on withdrawal as soon as the dressing has set, leave behind it in the

latter a hollow longitudinal ridge. This may be readily divided by a few strokes of a rasp at any time it is desired to remove the dressing, and, a clean, straight cut being thus made, the splint will be in suitable condition to reapply if necessary. As applied to the part, the chain presents the form of an inverted T; and, although the upright portion stands half an inch high, it increases the circumference of the limb only one-eighth of an inch, and does not affect the proper tension of the dressing.

When a hinge in the chain is required, two chains are applied,—the ridge formed by the one in front being cut, while that behind makes the joint. If the chain has to travel over a very convexed curve, it should be wrapped in wax-paper, in order to bridge over the spaces between the widely separated spines.

REPORT

OF

A CASE IN WHICH AN ENORMOUS GALL-STONE WAS REMOVED POST MORTEM.

By

J. H. MUSSER, M.D.,

CHIEF OF THE MEDICAL DISPENSARY IN THE UNIVERSITY OF PENNSYLVANIA.

[Read January 7, 1885.]

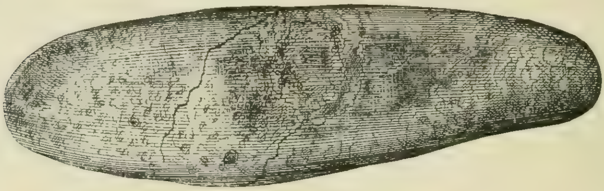
I DESIRE to present, for T. M. Livingstone, M.D., of Columbia, Pennsylvania, an enormous gall-stone which was removed at the autopsy of a female, aged 66 years, who died of colloid cancer of the omentum. The carcinoma was recognized during life, the presence of the gall-stone not suspected. In addition to the malignant disease of the omentum, secondary growths were seen in the bile-ducts. One nodule completely occluded the common duct. The gall-bladder was firmly contracted around the stone, which is exhibited to-night, and a small fistula at the fundus communicated with its cavity and that of the peritoneum.

A full report of the case by Dr. Livingstone may be found in the Lancaster *Practitioner* for December, 1884.

The stone weighs 394 grains (or 25.6 grms.), and is three and one-third inches (or 8.5 cm.) long. The largest circumference measures 7.9 cm. The small end, corresponding to the cystic duct, measured 5 cm. in circumference; the large end, 7.1 cm.

It is elongated cylindrical in shape, tapered at each end. The end of the stone corresponding to the neck of the gall-bladder shows the greatest reduction in its circumference, as seen by the measure-

ment, and evidently extended into the cystic duct, as one-half inch from the point it is slightly curved. In the middle of the stone an abrasion, so to speak, not due to handling, is seen, the size of a five-cent piece. It shows three or four laminæ of the stone. Both ends of the stone are rough, the body quite smooth. The accompanying cut from a photograph, by Dr. G. A. Piersol, is presented to show the



Natural size. Weight, 394 grs.

outline of the stone. On section, a nucleus the size of a filbert is seen, crystalline in character, and made up of cholesterine. The remainder of the stone is made up of bilirubin, deposited in laminæ. It is not unlikely the cholesterine nucleus was an old stone, the bilirubin deposition being quite recent. Dr. Livingstone sends the stone to be deposited in the Mütter Museum.

FACTS SERVING TO PROVE THE CONTAGIOUSNESS OF TUBERCULOSIS; WITH RESULTS OF EXPERIMENTS WITH GERM TRAPS USED IN DETECTING TUBERCLE-BACILLI IN THE AIR OF PLACES OF PUBLIC RESORT, AND A DESCRIPTION OF THE APPARATUS.

By

W. H. WEBB, M.D.

[Read February 4, 1885.]

It is in accord with the spirit of the age to attempt to get at the root of all things affecting the health of our race. The causes of tuberculosis and influences modifying the progress of the malady claim not only the active interest of those engaged in the field of scientific medical investigation, but the attention of every mind that fully appreciates the universal prevalence and immense mortality resulting from the scourge consumption. It may add somewhat to the interest, if not to the elucidation, of this subject to trace the progress of thought springing from the observation of clinicians of all ages. By collecting the expressions of the *master-workmen* of different times, we will see that knowledge upon the subject has been steadily progressive, and that what now seem to be proven facts, have been preceded by flashes of truth in almost every epoch of medical history. The discovery of the tubercle-bacillus was not

drawn from the inspiration of genius, but from the shaping of clinical facts gathered in the progress of our art, from the time of the Father of Medicine until the day that Koch discovered a peculiar microbe in tuberculous patients.

The word tubercle was used by Hippocrates,[1] but he applied it principally to designate small external tumors —phymatæ—as, hordeolum, furunculus, sycosis, anthrax, wheals, etc. It is said that the word was used by Celsus, about A.D. 20, but no special meaning was attached to it by him.

Franciscus Deleboe Sylvius, who lived between 1614 and 1672, was not only a strong advocate of the doctrines of Hippocrates, but also did much to advance the knowledge of this disease. He was the first to use the word tubercle in designating the hard nodules found in the lungs of the phthisical. He was also the first to speak of the formation of cavities and destruction of the lung tissue by the softening and breaking down of these hard masses. He describes three kinds of consumption —one of blood; one of the lungs, occasioned by bad nutrition; and one of degenerated glands. Through this latter proposition he may be regarded as the originator of the theory of a relationship between consumption and scrophulosis. "He believed that in the predisposed the disease may be excited by contagion."[2]

As a result of autopsies made by Willis, and also by Bonetus, about from 1640 to 1670, the subject of phthisis was very much advanced.

[1] "The Genuine Works of Hippocrates." Published by the Sydenham Society. London, 1849.

[2] Quoted in "A Practical and Historical Treatise of Consumptive Diseases." By Thomas Young, M.D. 1815. P. 178.

When Richard Morton's[1] celebrated work appeared, the opinions set forth therein were not only greatly in advance of those of his own time, but were destined to supplant all others, and to be accepted as correct for more than one hundred years after his death. He asserted that all consumption originated through tubercles, and that they gave rise to the dry cough. The idea entertained by Hippocrates, that consumption was due to inflammation and ulceration, he strongly opposed. He also declared that he believed the disease to be propagated by infection. "For," said he, "this distemper—as I have observed by frequent experience—like a contagious fever, does infect those that lie with the sick person with a certain taint." (P. 67.)

Desault[2] also insisted that tubercles were the essence of consumption, and that ulceration of the lungs and hæmoptysis were the result of the deposit. He believed in the contagiousness of the disease when ulceration had occurred. A statement, which at this day seems to have been almost prophetic, occurs in his writing upon tuberculosis: "Worms," he declares, occasioned by the putrefying lungs, "propagate the disease and cause it to spread."

From the time of Morton to 1793, the subject of tuberculosis commanded the attention of some of the most distinguished men in medicine. (Sydenham, F. Hoffmann, Boerhaave, Van Swieten, Sauvages, Morgagni, Cullin, Hufeland, Portal, Stark, Ruysch, Stahl, Reid, and Baumé.) At this time Baillie's great work appeared,[3] in which he demonstrated the existence of tubercles in other organs besides the lungs.

1 "Phthisiologia, or a Treatise of Consumption." 2d Ed. London, 1694.

2 Quoted by Thomas Young, M.D. Loc. cit.

3 "Morbid Anatomy." London, 1793.

Bayle,[1] an independent worker as well as thinker, insisted most strenuously that phthisis is a general chronic disease, and owes its origin to a special principle—the tuberculous. He, too, denied most positively the teachings of Hippocrates, that consumption was due to inflammation and ulceration. To him is due the credit of discovering what is now known as miliary tuberculosis. "Out of nine hundred autopsies performed by him, he found 624 had tubercular phthisis, 185 the granular or miliary tubercle, 72 melanotic, 14 ulcerous, 4 the calculous, and 3 the cancerous." He says further: "This disease appears always to depend on a peculiarity of constitution. Hæmoptysis is a frequent symptom of consumption, and is sometimes mistaken for its cause; but it often happens that when hæmoptysis has been fatal, the lungs are found full of tubercles."

Laennec, who followed Bayle, declared that all consumptions, including scrophulosis, were nothing but the consequences of the tuberculous specific principle, which might be inherited or be acquired. In demonstrating his theory he made use of auscultation, which it is said he originated, in accurately determining the diseased condition of the lungs. Laennec's views in regard to the pathology of consumption, notwithstanding he had very strong opponents among his colleagues, were held for a long time. Schönlein, in the main, held with Laennec, but differed most positively with him in making a marked distinction between tuberculosis and scrophulosis. At this time scores of eminent investigators were busy with this subject, and the confusion of ideas that then existed occasioned the promulgation

[1] "Recherches sur la Phthisis." Paris, 1810, p. 66. Quoted by Dr. Young. Loc. cit., p. 452.

of many widely divergent theories; thus were described tuberculization of pus, tuberculous pus, gray tubercle, yellow tubercle, gray infiltration, tubercle granules, tubercle corpuscles, granulosis, albuminous tubercle, etc. Indeed, the number of forms assumed by this disease was limited only by the number of writers upon it. This tended to give to the simplicity of the theories advanced by Laennec a great attraction to many, who held to them from sheer comfort of mind. Out of this dire confusion a way was opened by Virchow, whose cellular pathology gave us a positive science by which the theories of previous writers were exploded.

In the early part of 1882 Dr. Robert Koch made his name immortal by giving to the world the result of the researches and experiments[1] by which he swept away all false ideas that had existed in regard to tuberculosis for a period of over two thousand years. It was then that he made the announcement that "tuberculosis is a specific, infectious disease, caused by a specific micro-organism—the bacillus tuberculosis—which constitutes, in fact, the true tubercle virus." This statement is one of the most remarkable, in its import, in the history of medicine.

Koch reached this position by the results obtained from experiments made with the tubercle-bacilli which he had artificially cultivated. He prepared a nutritive substance and introduced into it a speck of pus taken from a tuberculous human lung. In this way he obtained a number of bacilli, with which he infected fresh material, and by frequent repetition of this process, which he carried on for many months, he succeeded in

[1] Die Etiologie der Tuberculose. Berliner klin. Wochenschrift, 1882, No. 15.

obtaining bacilli very many generations removed from those taken from the diseased lung. These cultivated bacilli were introduced into the circulation of healthy animals, and in every instance induced tuberculosis. Tubercles in large numbers were found in the lungs, liver, and spleen of all the animals thus experimented upon.

The labors of the illustrious Pasteur, of France, and of Koch, of Germany, are now well known to us all. They are the leaders of a host of equally zealous investigators who have acquired more or less distinction through their efforts in this direction.

Villemin,[1] Buhl,[2] Bollinger,[3] Fraentzel,[4] Balmer,[5] Ruhle of Bonn,[6] Lichtheim,[7] the late Prof. Cohnheim,[8] Gaffky,[9] Ewald,[10] Ehrlich,[11] Kowalski,[12] Wilson Fox,[13] Cheyne,[14] Shakespeare,[15] Sternberg,[16] Ernst,[17] Colin,[18] Tappeiner,[19] Williams,[20] and others, who form a legion of self-sacri-

[1] Gazette Méd. de Paris, Dec. 1865. Also "Études sur la Tuberculose." Paris, 1868.

[2] Lungenentzündung, Tuberculose, und Schwindsucht. 1873.

[3] Archiv f. Experim. Pathologie, Bd. I., 1873. Also N. Y. Med. Record, March, 1884.

[4] Berliner klin. Wochenschrift, 1882, No. 45. [5] Ibid.

[6] Medical Record, New York, May, 1883. [7] Ibid.

[8] "Consumption as a Contagious Disease," London, 1880. Translation by H. D. Cullimore.

[9] Report of the Imperial Health Office, Berlin, 1884. See Review of Amer. Journ. of the Med. Sci., July, 1884.

[10] Medical News, Philadelphia, Sept. 6, 1884, p. 275.

[11] Deutsche med. Wochenschrift, No. 19, 1882.

[12] Wiener medizinische Presse, Feb. 24, 1883.

[13] Med. Times and Gazette, London, 1883, vol. ii. p. 672.

[14] Practitioner, London, 1883, vol. xxx.

[15] Proceedings of the Phila. Co. Med. Society, 1884, pp. 315, 320.

[16] Medical Record, New York, October, 1884.

[17] Amer. Journ. of the Med. Sci., October, 1884.

[18] Med Centralblatt, 1873, No. 30. [19] Virchow's Archiv, Bd. 82, 1880.

[20] The Lancet, London, February 24 and July 28, 1883.

ficing, earnest, and conscientious workers, banded together in the interests of science and of their fellowmen, and inspired by the hope of being able at some future day to stay the progress of a malady which has been the occasion of more deaths than all the epidemics of disease, and all the disasters by land and sea, not only command the attention and support of the scientific world, but also the gratitude of every intelligent human being.

With a rapid but steady pace those observers are advancing on the road which will soon lead to the desired goal. The clouds of errors are being dissipated by newly discovered truths, and to-day the subject of tubercular phthisis is better understood than ever before. It is my purpose this evening to bring to the notice of the Fellows of the College some facts by which, I think, the contagiousness of tuberculosis is clearly demonstrated.

Careful researches by De Quatrefages,[1] Cook,[2] Livingston,[3] Rush,[4] Budd,[5] and others, seem to prove that tuberculosis first appeared among the inhabitants of Europe, and gradually manifested itself in those parts of the world with which they had intercourse. If this is true, it is one of the best evidences of the contagiousness of phthisis.

A contagious or infectious disease can have but one cause, and this is eminently true of tuberculosis, which does not arise from a variety of causes, but is solely due to the tubercle-bacillus. Wherever this bacillus finds its proper nidus it will there develop and multiply; and, if this should be in living animals or human beings, the

[1] The Human Species, by A. De Quatrefages, N. Y., 1883, pp. 428, 430.

[2] Ibid. [3] Ibid. [4] Medical Inquiries and Observations, Phila., 1789, p. 137.

[5] The Lancet, London, 1867, vol. ii. pp. 451, 452.

progress of the disease will be determined by the character and amount of food offered for the growth of this germ; thus with a nidus rich and plentiful we may have a case of acute phthisis lasting not more than thirteen days;[1] and, on the other hand, if the pabulum is poor and scant, the case may be a chronic one extending over a period of twenty-five years, such a case having occurred in my own practice.

The bacilli may enter the system through the lungs or by the stomach. The air we breathe, as well as the food we take, especially in the vicinity of the phthisical, may be laden with these germs. The air of the ventilating flues at the Brompton Hospital, when carefully examined, was found to contain tubercle-bacilli in fair abundance.[2] The sputa of tuberculous patients drying upon our streets is ground into an impalpable powder by the feet of pedestrians, and is then disseminated through the air to be inhaled alike by the healthy as well as those predisposed to tuberculosis. Such sputa, mixed with the dirt of the streets, have been collected, dried and powdered again, at frequent intervals during a period of several months. Guinea-pigs were then inoculated with this matter and in a short time the animals thus treated died from tuberculosis.[3]

To admit that the tubercle-bacillus is a pathological product is to express a belief in spontaneous generation,[4] and I feel sure that none of my enlightened hearers is prepared to subscribe to that doctrine.

[1] Medical Diagnosis, by J. M. DaCosta, M.D., LL.D. 6th Ed. Philadelphia, 1884, p 320.

[2] The Lancet, London, July 28, 1883.

[3] Med. and Surg. Reporter, Phila., 1884, vol. i. p. 697.

[4] Floating-matter of the Air, by John Tyndall, M.D. New York, 1882, pp. 277, 320.

It is asserted, by some pathologists, that other matter or irritant than the tubercle-bacillus is capable of producing the disease. This idea is not a new one, for Richard Morton says: "Chalky stones that are preternaturally bred in the lungs; or nails, and other hard bodies, slipping down into the lungs, when persons laugh, are to be recorded among the causes of a consumption of the lungs;"[1] and he narrates a case, p. 247.

It is also claimed by a number of writers, that certain callings or occupations may be a cause of tuberculosis, owing to fine particles of dust inhaled by those employed. Thus coal-miners, dry grinders, stone-cutters, moulders, operatives in cotton and woollen mills, etc., are apt to have the disease. But those who believed that the dust breathed by individuals engaged in these occupations might occasion phthisis were evidently oblivious of the fact that the air carried, in the form of germs, far more potent factors; and that while the dust may have produced an irritation of the air-passages, the presence of the tubercle-bacilli was essential to the production of the disease. The inhalation of irritants, or lowered vitality occasioned by certain occupations, may cause the predisposition, but they are never the cause of the disease *per se.*

Not all the predisposing causes united could in any instance induce tuberculosis without the advent of the tubercle-bacillus. That something more is needed was admitted by Pollock twenty years ago, when he declared that there must be "some subtle agent to precipitate, concentrate, and shape these elements of disease into

[1] Loc. cit., p. 67.

tubercle."[1] And Da Costa says, "whatever it be, is something special."[2]

Experiments have demonstrated, beyond doubt, that it is impossible to induce true tuberculosis in any case where proper precautions have been taken to remove from the irritant used all living germs. This is now accepted as a fact by many of those who once held a contrary opinion. Wilson Fox, Cheyne, Sternberg, and others, who performed these experiments under the conditions mentioned, have acknowledged that under such circumstances it is impossible to produce the disease.

Objections are also made to the fact that these bacilli are the cause of tubercle, because they were not found in all the cases of tuberculosis examined by certain investigators. It is fair to presume that in these instances they must have escaped detection, since bacilli have been found in every case of tuberculosis examined by careful observers.

Many instances are recorded where foreign bodies have been carried into the lungs by gunshot wounds or otherwise, without occasioning much disturbance in the parts or seriously affecting the health.

Rush,[3] with his experience in the Revolutionary War, declared that he had never known a case of phthisis to result from wounds in the lungs, and this observation was supported by the Surgeon-General of the Royal Army.

[NOTE. That the excretion of these bacilli might prove to be the *materies morbi*, was suggested by me some time ago; and this opinion is also entertained by Dr. G. M. Sternberg, U. S. A., who subsequently made the same suggestion in the *Medical Record*, N. Y., Oct. 25, 1884.]

[1] The Elements of Prognosis in Consumption, London, 1865, p. 337.

[2] Phila. Med. Times, June 19, 1880.

[3] Medical Inquiries and Observations, Phila., 1805, vol. ii. pp. 72, 73.

A number of cases of gunshot wounds of the lungs occurred during the late war, but, as far as known, they were not the occasion of any death by phthisis.[1]

I am free to admit that, in cases where a predisposition exists, it may be still further developed by the presence of an irritant, just as a furuncle in one individual may be harmless, and in another the starting point of a cancer. The late General Baxter,[2] of this city, received a wound in the lungs on the 6th of May, 1864, and was more or less actively engaged in his duties until twelve years afterwards, when, during a fit of coughing, he ejected what appeared to be a hardened bit of pus. This, upon examination, proved to be the envelope of a small piece of coarse, red cloth, half an inch in diameter (such as is used for the stiffening and padding of coats), which had been carried into his lungs at the time he received the wound in 1864. During all this interval there had been a constant suppuration of the lungs, occasioning considerable discomfort, but not sufficient to render him unable to fill several important positions demanding his careful attention. Three years after expelling the foreign body (seventeen years from the time he received the wound) he died, it is said, from phthisis. In this instance, admitting that he died from phthisis, a predisposition to the disease was evidently established by a greatly lowered vitality, occasioned by the long-continued suppuration. For twelve years he lived without a sign of phthisis; but after he had rid his lungs of the original irritant, the bacillus tuberculosis found its way to the rich soil so long prepared for its

[1] Med. and Surg. Hist. of the War. Second issue, 1875, Part I., Surg. vol., pp. 478, 481.

[2] The Daily Evening Telegraph, Philadelphia, May 10, 1881.

reception, and there multiplied until the life of the individual was ended.

So certain diseases, occasioning an irritation or a lowered vitality of the pulmonary mucous membrane, have the reputation of being the indirect causes of tuberculosis. Measles, especially when occurring in children of phthisical parents, are liable to have consumption as a sequel. The mucous membranes are implicated in this disease, probably more so than in any of the eruptive fevers; the epithelium is cast off, and the denuded membrane exposed to the direct contact of the tubercle-bacillus.

It has been satisfactorily demonstrated that tuberculosis may be caused by inoculation in the human subject. Laennec, whilst examining a vertebra containing tubercle, slightly wounded one of his fingers with the saw blade. In the site of this wound a small, rounded tumor subsequently appeared, which, upon investigation, exhibited all the physical characters of tubercle. It was destroyed by the application of an escharotic, and no bad effects resulted from it.[1]

Another instance was that of a fisherman free from tuberculosis, but suffering from gangrene of the toe, who was purposely inoculated. Thirty-seven days after the experiment he died, and the autopsy revealed a tuberculous deposit in the lungs and liver.[2]

But the most satisfactory evidence of the effects of inoculation of tubercle is that presented in the case recorded by Dr. E. A. Tscherning.[3] The subject, em-

[1] Diseases of the Chest. Translated by J. Forbes, M.D. London, 1834, p. 305.

[2] Gazette Méd., 1872, p. 192. Quoted in Biennial Retrospect of Med. and Surg., 1871–72, p. 38.

[3] Hospitals-Tidende, Copenhagen, Dec. 17, 1884.

ployed as cook in the family of Prof. H., was a perfectly healthy woman, about twenty-four years of age, with a history unexceptionally free from any hereditary taint of scrofulous or tuberculous affections. After a short illness the Professor died of phthisis. The cook unfortunately broke the glass sputum-cup used by her employer, and a spicula from it punctured one of her fingers. Fourteen days afterward there appeared at this point what seemed to be a felon. This was treated at Prof. Studgaard's clinic, and at the end of a few weeks the finger was much better; a little nodule, however, about half as large as a pea, was found to exist in the subcutaneous connective tissue, which after awhile became tender and œdematous. This was now cut out, and the wound healed readily. About three months from the time of the accident, Prof. Studgaard found that the sheath of the tendon was thickened, and two cubital and two axillary glands were enlarged. He disarticulated the middle finger, and the tendon, with its thickened sheath, as well as the enlarged glands at elbow and axilla, were dissected away. Upon examination, the sheath of the tendon was found to be filled with pale granulations. Sections of the sheath and of the extirpated glands were subsequently subjected to microscopic investigation, and numerous tubercle-bacilli found in them, which positively established the peculiar character of the affection.

Dr. Tscherning has observed upwards of thirty cases of localized tuberculosis, and in each instance the microscopical appearances were the same as in this case.

Many eminent men, by their constant attendance upon the phthisical and by their close and frequent study of the post-mortem conditions of their cases, have been

made victims to the disease themselves. Among those who have met death in this way may be mentioned Bayle, Laennec, Delaburg, Dance, Young-Thomas, and many other names could be added.

Much stress has been laid upon heredity as being one of the chief causes of the vast mortality from this disease. It is my belief that phthisis is never transmitted from parent to child; it is simply a predisposition that is inherited. By predisposition I mean a greatly lessened power of resistance in the tissues, especially in the lymphatic system. If tuberculosis was inherited, we might expect to find some indications of this in the fœtus, but the observations of Guizot,[1] Gluge,[2] Heller,[3] and Virchow,[4] have shown that this is not the case. In 1300 fœtuses examined by Heller there were no evidences of a tuberculous taint in any one of them, notwithstanding the fact that in one instance the patient died of phthisis with the fœtus in utero. Virchow, with his experience of more than fifty years, says: "He had not seen a single case of direct transfer in the fœtus." This also holds true in regard to the offspring of animals which have been under observation while suffering from tuberculosis; there is no instance on record in which they have exhibited a trace of the disease.[5]

The presence of the disease in infants is undoubtedly due to the bacillus, or its spores, contained in the milk of the phthisical mother, or in the air it is constantly obliged to breathe.[6] In other words, the disease is not transmitted, but it is acquired. I have elsewhere

[1] Quoted by Dr. Durant. Trans. of the N. Y. State Med. Soc., 1878, p. 174.
[2] Ibid. [3] Medical News, Phila., 1884, p. 302. [4] Ibid.
[5] Practitioner, London, 1883, vol. xxx. p. 318.
[6] British Med. Journ., 1879, vol. xi. p. 619.

shown the fallacy of the hereditary transmission of the disease.[1]

There are but few insurance companies that will accept as a risk anyone whose family history is not clear of tuberculosis; hence it would seem that such careful exclusion would remove all questions of hereditary transmission in those losses which they may sustain by deaths from phthisis. One of the most conscientious companies in this respect is The Mutual Life Insurance Company, of New York. It is especially careful in excluding such risks, and will not only refuse to accept an applicant who has a phthisical history, howsoever remote, but will not regard an application in which there is the least evidence of a predisposition to the disease, no matter what the age of the applicant may be. Notwithstanding the exercise of an unusual amount of vigilance, they are nevertheless obliged to declare that "consumption has been the occasion of more deaths than any other disease, giving a percentage of $17\frac{61}{100}$ of the total mortality; while deaths recorded under other headings, but properly belonging to this, would swell the number to 20 per cent."[2] Here, then, is a freedom from an hereditary taint as far as rigid examinations are capable of determining it. Under such circumstances the rate of mortality is surprising, to those, at least, who have faith in the hereditary transmission of the disease. A few years previous to that in which this report was made, the death-rate from consumption in the adult male population of New York City was $30\frac{17}{100}$

[1] "Reasons for Believing in the Contagiousness of Phthisis." Read before the Philadelphia County Med. Society, June 11, 1884.

[2] Preliminary Report of the Mortality Experience of The Mutual Life Insurance Company, of New York. New York, 1875, p. 12.

per cent. This is but little over 13 per cent. of the deaths among those who were considered especially exempt from the disease.

It would be impossible to enumerate the so-called causes of tuberculosis. The disease has been attributed to every imaginable influence which could occasion a morbid condition of the system. This error is readily accounted for when it is understood that any influence which will bring about a lowered vitality of the body will induce a predisposition to the disease, which is established by the presence of the bacillus tuberculosis.

Age.—In looking over "health reports" and other statistics I have been surprised at finding records relating to the time of life when tuberculosis is most prevalent, which are entirely at variance with the ideas entertained by many practitioners. It is very generally believed that at the age of puberty, especially in those supposed to possess hereditary taint, phthisis is most apt to manifest itself, and that the liability to contract the disease is lessened with advancing years. It seems, however, that this is not the case, for in early childhood and at puberty the mortality is less than at any other period of life. There is reason for believing that the best way to determine the time of life at which the disease is most fatal is to compare the death-rate occasioned by it at certain periods of life with the number of living persons at the same age. This has been done by several reliable persons, including Dr. Edgar Holden,[1] and I will read to you an interesting table prepared by A. Wuerzberg, the librarian of the Imperial Health Office at Berlin, Prussia.[2] This table is as follows:

[1] The Medical Record, New York, July 12, 1884.

[2] Amer. Journ. of the Med. Sci., July, 1884, p. 192.

Of 10,000	individuals	aged 0– 1	year there	die annually	of	consumption	$25\frac{45}{100}$
"	"	" 1– 2	"	"		"	$20\frac{41}{100}$
"	"	" 5–10	"	"		"	$4\frac{66}{100}$
"	"	" 15–20	"	"		"	$18\frac{37}{100}$
"	"	" 20–25	"	"		"	$30\frac{24}{100}$
"	"	" 25–30	"	"		"	$36\frac{73}{100}$
"	"	" 30–40	"	"		"	$41\frac{12}{100}$
"	"	" 50–60	"	"		"	$67\frac{94}{100}$
"	"	" 60–70	"	"		"	$93\frac{18}{100}$
"	"	" 70–80	"	"		"	$61\frac{72}{100}$
"	"	over 86	"	"		"	$25\frac{80}{100}$

This table goes to show that at the two extremes of life, where vitality is at the lowest, thus lessening the power of resistance, the disease is most fatal.

It must not be forgotten that, among the many causes which lead to a predisposition to tuberculosis, conditions of mental depression play an active part. Bad habits or immoral conduct, which lead to bitter regrets; or domestic infelicity, occasioning long-continued fret and worry, will produce a depression of this character more or less marked. A case recently came under my notice in which the patient's health was affected by the unfortunate condition of his domestic affairs; they were the occasion of continued anxiety and worriment for several years, and finally brought him into a condition of nervous exhaustion. This was soon followed by the signs of phthisis, and he died about six months afterwards. In this case the predisposition was certainly due to a lowered vitality, induced by long-continued mental depression, aided somewhat by the patient's occupation, which was that of a bookkeeper. There was no hereditary tendency to the disease, and, had he been more fortunate in his domestic relations, he might still be living.

The following interesting cases, which I have personally investigated, will go to support some of the assertions I have made in my paper:

Case I.—J. E., an invalid, was married; twelve months afterwards his wife gave birth to a child, and in the following month the father died of phthisis. At the age of five months the child died of marasmus, and in sixteen months after her accouchement the wife died of phthisis. She was of a healthy and long-lived family, but had occupied the same room with her husband during his illness.

Case II.—S. Y. was a healthy young man, who married a lady that was physically below par. About a year after marriage she gave birth to a child, and from this period onward she declined in health, and ultimately died of phthisis five years afterwards. Eighteen months prior to her death her husband exhibited symptoms of tubercular laryngitis, and died of consumption four weeks before his wife. In this case I ascertained that the wife had come from a tuberculous family (her parents and five sisters having died from the disease), while in the husband's family there was not a trace of tuberculosis, his parents living far beyond the allotted threescore and ten, and his brothers and sisters in the full enjoyment of health. This gentleman, who was greatly devoted to his wife, had been constant in his attendance upon her, and had slept in the same room.

Case III.—I. R., a young man, aged 27 years, of very temperate and regular habits, who presented no family history of tuberculosis, and whose constitution and general health were excellent, married a young lady of delicate health, in whose family consumption had caused the death of father, mother, and three sisters. The occupation of the young man was that of ticket agent in a railroad office. About three and a half years after his marriage he became ill, and a year after the commencement of his illness died of phthisis. Two years and a half subsequent to his death his wife died of the same disease.

In the first case narrated to you the healthy, robust bride certainly contracted the disease from her husband. In the second case the young man, with an excellent family history and in good health at the time of his marriage,

not only contracted the disease from his phthisical wife, but died of it four weeks before she did. In the third case we have a healthy and vigorous young man, with an excellent family history, marrying a phthisical girl from a phthisical family, and what is the result? Through his close companionship with his consumptive wife he contracts the disease which occasions his death two and a half years before his wife succumbs to the malady.

I might add to this list a number of similar cases, were it necessary to do so, for I have the notes of many which prove conclusively the contagiousness of phthisis. Indeed, any one can lay his hands upon recorded cases without number which would convert even the most sceptical to this belief.

No one, not even the non-contagionists, can declare that the cases I have narrated are simply coincidences. The experience of medical men, especially of those who are engaged in the treatment of lung disorders, must be similar to my own, and I cannot see how there can be a question in their minds in regard to the contagiousness of phthisis.

If the most convincing proof of the truth of a comprehensive theory lies in its power of absorbing and finding a place for new facts, and its capability of interpreting phenomena which had previously been looked upon as unaccountable anomalies,[1] then I know of no theory more truthful than the one which I have advocated before you this evening. It will fully explain every phenomenon connected with this malady, the universal mortality it occasions in every part of the world,

[1] Contributions to the Theory of Natural Selection, by A. R. Wallace, London, 1870, p. 45.

and why one member of a family after another, with no hereditary predisposition, has succumbed to its power.

It is a singular fact that in all the recorded cases where the disease has been occasioned by close association with the phthisical, as in nursing, it has been unusually rapid in its course, frequently carrying off those unfortunates during the lifetime of those from whom the disease was contracted.

And what does all this teach us? Simply this, that our real strength in battling with this terrible disorder lies not so much in medication as in the application of hygienic and sanitary laws.

Surgeon-General Von Lauer, of the Royal Prussian War Department, in a letter dated October 16, 1884, kindly enclosed to me a copy of the instructions which he issued in regard to diseases of the lungs. They are of such importance that I quote them in full. It is also of interest to know that in Austria, where the bacillary origin of tuberculosis met with greater opposition than anywhere else in Europe, the government has recognized its infectious nature and has issued official instructions similar to these of Surgeon-General Von Lauer. The same precautionary measures should be adopted in the hospitals of our own country, and it is fair to assume that this will be done.

[Copy.]

DEPARTMENT OF WAR.

Berlin, Aug. 31, 1882.

The various detailed reports which, in pursuance with request of Nov. 24, 1881 (No. 157, II, M. M. A.), have reached this Department, have clearly shown that there exists no material difference of

opinion regarding the reasons for the high annual sick and mortality rate from consumption during the time of active service.

The universally acknowledged causative relations will necessarily lead to still greater caution in the treatment and care of those exhibiting the earlier symptoms of chronic pulmonary disease, as well as those in whom a predisposition is suspected or clearly discernible. The prescribed regulations should, therefore, be borne in mind, in order that the number of consumptives in the army may thereby be diminished. The following instructions must always be carefully observed:

1. Although the predisposition to affections of the lungs cannot be objectively determined, and the time permitted the surgeon during the recruiting service often not extended enough to permit a careful and searching examination, to determine this question, the medical officer in charge is earnestly urged to consider the build, configuration, and exhaustibility of the thorax. In this connection he is to adhere closely to the instructions of April 8, 1877, regarding the normal limitations. Shoemakers and tailors (Oekonomiehandwerkern) of delicate frame require very careful inspection of the chest organs.

When the circumstances attendant upon the recruiting service are not favorable to exact examination, special attention is to be paid to more rigid inquiry when the recruit reaches his regiment, as directed in Par. 13 of the instructions. Here it will be of value, in forming an opinion in your cases, to seek direct official information regarding family history or previous disease of the lungs or pleura.

But in order not to lose sight of those cases which have either been overlooked at the first inspection, or whose character could not then be ascertained, the recommendations of the various corps surgeons that, with the coöperation of the proper authorities, medical examinations should be repeatedly made at stated intervals, should be particularly borne in mind, with special attention directed to those in whom disease of the respiratory organs is suspected. Special records, carefully noting the condition at each examination, must be kept. The extent to which the weakly are to be spared the arduous work of training must be determined by the requirements of individual cases. The industrious use of douche baths, to harden the skin and accustom to exposure, naturally suggests itself here.

2. The attention of surgeons is directed to the fact that the instructions (Par. 5, Sec. 4, to Par. 7, Sec. 2) do not permit a judgment upon volunteers without considering their fitness for field service. Complete fitness, therefore, is indispensable to a declaration of efficiency.

3. For convalescents from acute diseases of the respiratory organs a prolonged period of after-treatment and care is desirable. If the circumstances of the patient make home attention attainable, and only then, a lengthy furlough is to be recommended. Those returning from such furloughs are to be carefully reëxamined, and, if necessary, their transference to the appropriate health resort taken under advisement.

4. That the first symptoms of disease of the lungs may not be overlooked in making the round of the barracks, particular attention should be paid to apparently mild "catarrhs," utilizing, if necessary, evening temperature measurements. Doubtful cases should be transferred to hospital for observation.

5. The opinion of many medical officers, that prompt measures should be taken for the discharge of sufferers from chronic pulmonary disease, should not be forgotten. That even one attack of hemorrhage (Bluthusten), if it is proven to be of undoubted pulmonary origin, is sufficient cause for discharge, and is especially emphasized. That the early dismissal of cases affording no probability of usefulness to the service removes a source of infection for hospital and barrack, must be viewed as by no means the least important advantage of this provision.

Now that experimental pathology has furnished exact scientific corroboration of the theory of the infectiousness of phthisis, more importance than ever must be attached to the separation, both in hospital and barrack, of those afflicted with or suspected of phthisis, from other patients, especially from those suffering with inflammation of the lungs or recent bronchial catarrh. The sputum being the principal carrier of the disease germ, and consequently the principal source of infection, provision for its removal and disinfection (Unschadlichmachung) follows as a matter of course.

In answer to the question raised by this Department, as to whether new measures for the diminution of the number of cases of

phthisis, with particular reference to the necessity for the establishment of climatic summer or winter stations for their treatment, were called for, the responses were unanimous against such establishment. The indications for them were considered uncertain, and the existing provision adequate for the present necessities of the army. The Department endorses this view, and is convinced that the careful observance of the general directions herewith transmitted will be of interest and service to the army as well as to the patient. Although tedious attempts at cure by long-continued stay at climatic stations may be considered of doubtful value to the phthisical patient, and not at all likely to furnish the army with a soldier fit for field service, the prompt despatch of a convalescent from an acute non-phthisical affection of the respiratory apparatus to an appropriate station, is warmly to be commended. Such station, from among those at the disposal of the Department, is to be carefully selected, and treatment conscientiously carried out.

This communication, with five copies, is transmitted to you, with the request that you submit your views to the General commanding, and instruct the sanitary officers of the corps to be guided thereby.

[Signed] V. LAUER-STRUBE.

Department of War, Army Medical Division.

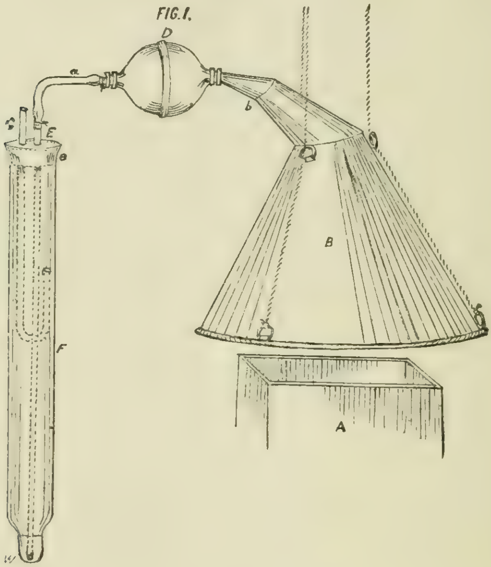
To all Royal Corps Surgeons, No. 230, 4, 82, M. M. A.: 64, 9–84, M. M. A.

Like other disease germs the tubercle-bacilli are carried by the air, and will, of course, be found to be more plentiful in the vicinity of the victims of tuberculosis. A single bacillus may as surely induce the disease as the presence of a great number; and, since we are at no time free from the chance of inhaling this germ, our safety lies in avoiding a "predisposition" to lung troubles. In order to determine whether the bacilli might be readily found in the air of the street, or of places of public resort, I had constructed the instruments which I now show to you.

The first apparatus I had made was after the plan of

the ordinary inhaler (as shown in the accompanying engraving, Fig. 1). The long tube (E) passed into a little well (W) at the bottom of the bottle containing glycerine, which was intended to retain any germs carried by the air passing through it; by rotating the bottle its sides were also smeared with glycerine, to give a still larger surface of glycerine for the contact of the air,

Fig. 1.

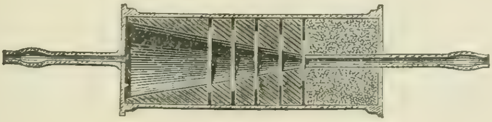


which, after entering the funnel (B), was forced through the apparatus by using the pump (D). This was undoubtedly an effective germ-trap, but the impossibility of drying the glycerine, which it was necessary to do in order to obtain microscopic proof of the presence of bacilli, obliged me to devise another method of obtaining them.

The second trap consisted of a brass cylinder contain-

ing a series of snugly fitted steel disks (as shown in the illustration, which gives a sectional view, Fig. 2). Each disk was perforated in such a manner that, when placed together, the openings formed a cone. Between each of these disks, across their openings, thin layers of pyroxylin were placed; the disks were then introduced into the cylinder, which was tightly fastened. To one end of the cylinder the pump connection was affixed, and the other end was connected with the funnel, which was placed over the ventilating flue, to take the air. When the pump was put in motion it drew the air through the apparatus, and necessarily through the veils of pyroxylin held in position by the metal disks, the pyroxylin thus serving to intercept the passage of any germs.

Fig. 2.



With this apparatus I visited a number of places of public resort, and, through the courtesy of those in authority, I was given free access to the parts of the establishments wherein the exit flues were located. These flues, in all instances except one, were placed in the ceiling of the auditorium, and directly over the audience. Here I placed the funnel-shaped extremity of the apparatus, and its pump was kept continuously in motion until fifteen or twenty minutes after the audience had retired. This experiment was repeated a number of times at each establishment I visited. The trap was then dismantled; the thin layers or veils of pyroxylin were removed from between the steel disks, and placed in the

hands of Drs. E. O. Shakespeare and Morris Longstreth for microscopic examination. These skilful microscopists have made the following reports:

PHILADELPHIA, February 3, 1885.

DEAR DOCTOR: The specimens of pyroxylin (Nos. 1, 2, 3, and 4) which you sent to me to examine microscopically for the presence or absence of tubercle-bacilli, were variously treated. Nos. 1, 2, and 3 were separately dissolved in a mixture of absolute alcohol and strong ether. The collodion thus formed was handled in either of two ways: *a*, a thin film was deposited on a thin cover-glass, such as is used in mounting of microscopic objects, and was stained in the manner recommended by Koch for the demonstration of the tubercle-bacillus; or, *b*, the collodion was excessively diluted in a test-tube, by addition of relatively larger quantities of alcohol and ether, and then allowed to stand for some hours, in order that suspended portions might fall to the bottom. The fluid was then carefully drawn off. The sediment at the bottom of the test-tube was mixed with a drop or two of sterilized beef-peptone-fluid, such as I keep in stock for bacteria-culture use, and was spread in a thin film upon a cover-glass. This film was also treated in the manner above mentioned for the demonstration of the tubercle-bacilli. I had, however, considerable trouble in following these methods. There was great difficulty in decolorizing the film; many times this seemed quite impossible.

In these three specimens of pyroxylin I found no bacilli tuberculosis.

No. 4 I determined to treat in another manner. I employed two different methods: *a*. I took portions of the pyroxylin and stained them, as I would do sections of tissue in which I wished to seek for tubercle-bacilli, namely, in the manner recommended and practised by Koch, methyl-violet being the color used in the aniline oil mixture. These were subsequently mounted in balsam in the usual way without converting them into collodion. *b*. Other portions of the pyroxylin were stained with fuchsin as the color of the aniline oil mixture. After staining in the usual way, including the methyl-

blue as contrast color, the pyroxylin was placed on an object-glass slide for the microscope and converted into collodion by using a mixture of ether and alcohol. As soon as it was dissolved a thin glass cover was placed over it. This latter method, in my hands, was by far the most satisfactory.

In the portion of pyroxylin prepared by the "*a*" method, I found two objects, which, by their size, shape, and color, had they been isolated and seen in sputum, I would have taken for tubercle-bacilli, but these objects were attached to fibres of pyroxylin, which, in spite of the successive action of weak nitric acid and of strong alcohol, and in spite of the subsequent use of Bismarck brown as a contrast color, were also tinted violet. This observation must, therefore, be classed as negative, or, at least, doubtful.

In the portion of pyroxylin treated by the latter method, "*b*," I found, after painstaking search, one bacillus, which, on account of its size, shape, and quite characteristic color (bright red, the ground being blue), I had no doubt was a tubercle-bacillus. There were two other rod-like forms, which, in size and shape, appeared identical with tubercle-bacilli, but the color which they showed me was so indistinct that it could not be safely made out. I have to report, then, the finding of one tubercle-bacilli in the specimen marked No. 4. There were, of course, numerous other objects in all the specimens examined, but, as you wished only to know of the tubercle-bacillus, I have thought it needless to particularize concerning them.

Yours very truly,

E. O. SHAKESPEARE.

To Dr. W. H. Webb.

PHILADELPHIA, February 4, 1885.

DEAR DOCTOR: In compliance with your request, I enclose you the following report on the examination of pyroxylin from your germ-trap, in relation to the presence or absence of the bacillus of tuberculosis (Koch).

The material consisted of five small pledgets of cotton, contained in a small phial, sealed with paraffine.

The five portions were carefully kept apart and examined separately.

The staining method employed was that recommended by Koch: Aniline oil and fuchsin, bleaching with dilute nitric acid, washing with dilute alcohol, contrast stain with methyl-blue (in some slides), and washing finally in absolute alcohol. The only variation made in this method of mounting as usually practised was in using a dammar medium instead of Canada balsam, which I have employed, since I have found that the dammar hardens more rapidly than the other. The examination of the specimens can be made with the oil immersion lenses more promptly, without the risks involved in displacing the cover-glasses, should the oil come in contact with the mounting medium. It was found, by a preliminary examination, that four of the five specimens of cotton were not likely to furnish any number of bacilli, and further search among these four specimens was consequently abandoned.

The fifth specimen, labelled No. 1, engaged the sole attention of further examination, as it was composed of the cotton which first met the current of air as drawn through the trap.

The cotton was very much discolored by dust and other matter, particles of which could easily be shaken off from it. Care, however, was observed so as to lose as little as possible of these adhering matters.

The staining, bleaching, and other steps in mounting, were carried out by first placing the cotton in a watch-glass containing the aniline fuchsin stain, and allowing it to remain, tightly covered, for twelve hours. Portions of the cotton were then thinly spread on a cover-glass, and the subsequent steps of the operation carried on in this position. It has been usual, I believe, in examining gun-cotton, to detect the presence of objects capable of being shown by a differential staining, to convert the cotton into collodion by admixture of ether and alcohol. This method I avoided, in the chief examinations, as being essentially faulty, since if the bacilli should be present in a dried film of collodion it would be impossible for the staining agents to come in contact with the microörganisms buried in the depth of the film.

Very considerable difficulties and much tedious searches were encountered in the microscopic examination, owing to interlacing and overlying arrangement of the cotton fibres; for, although the strongest

pressure was placed on the covers which the glass would stand—and many specimens were lost in this manner—nevertheless the depth of the material presented a field of much confusion. The confusion was somewhat lessened, but not removed, by adding another step to the process of mounting, viz., by treating the cotton after staining and bleaching with a mixture of ether and alcohol, for the purpose of converting it into collodion. While this treatment dissolved the cotton fibres, still some fibres of flax and wool were left. It did not, of course, help the confusion due to large amounts of dirt particles which were present. It was hoped that by thus making a collodion of the gun-cotton, after the staining process was completed, some advantages might be obtained. Such, however, was not the case. The examination of the six slides from specimen No. 1 gave the following results :

Slide a, 1 bacillus, 1 doubtful.
" b, 6 bacilli.
" c, 3 "
" d, 1 bacillus.
" e, none.
" f, uncertain.

It is not intended to convey the idea that these were the only bacilli present. A very careful examination might reveal the presence of more organisms. For the uncertainty of the examination excuse must be found in the nature of the materials dealt with ; the impossibility of rendering the layer of material of uniform thickness, as can be readily done with sputa and with sections of tissue ; the very large amount of dust particles scattered through a layer of considerable thickness ; the fact, also, which I have not seen alluded to previously, that many fibres of cotton have in them clefts, which retain staining material in spite of bleaching ; many of these clefts closely approach in length and breadth the figures of the bacillus ; and, finally, the short time which I have been allowed for the work since the specimens were placed in my charge for examination.

Yours very truly,

MORRIS LONGSTRETH.

To Dr. W. H. Webb.

The layer or veil of pyroxylin, through which the air from the flues first passed seems to have stopped the passage of all germs and other atoms, and in this way acted as a trap, to the exclusion of the other veils of pyroxylin placed between the disks for that purpose. Unfortunately, the portion submitted to Dr. Shakespeare for examination was not of the first layer, and to this may be attributed his inability to find more than one bacillus.

Furthermore, the number of bacilli found by Dr. Longstreth in the minute particle of the material he examined seems to indicate the presence of vast numbers of these germs in the entire layer removed from the trap.

And now, in conclusion, I desire it to be understood that I have spoken, not so much to maintain a proposition as to reveal the truth; and that, in giving you the opinions of those who have beaten a path wherein we may the more easily travel, I have but done justice to a class of men equally endowed as ourselves to observe and to reason from cause to effect. I would also state that the aim of this paper is simply to emphasize facts, leaving you to deal with them as your wisdom may dictate. A careful analysis of the writings and investigations of those who have given special thought to the subject which I have treated reveals the fact that, since the time of Hippocrates, there has been a gradual but steady progress towards the grand beacon which now illuminates our way. The very slowness of the advance, the suspicion with which the announcement of every new development has been received, and the earnest criticism to which they have been subjected, insure the safety of our position to-day. Apart from the ocular demonstration of scientific investigations of modern times, and from

a purely clinical standpoint alone, the weight of evidence as to the contagiousness of tuberculosis must certainly be appreciated by you all. Even those who do not acknowledge it in words proclaim it by their manifestation of doubt and quiet avowal that there is something lacking which will enable them to fix upon the cause of a disease maintaining such marked characteristics from age to age, and among all people.

We are living in a scientific age, and the medical profession is thoroughly imbued with its spirit and import. We deal with facts, and are little inclined to give heed to that which is purely speculative. Such superstitions as the "Royal Touch" belong to a departed age. "Coincidences" and "Happened so's" serve no longer to answer our inquiries concerning the causes or nature of disease. Never before have we been so well established in respect to the means and methods of making research and experiments in the domain of medicine, and never before have the searchers after its truths been more earnest in their efforts, or more hopeful of grand results. The discovery of the tubercle-bacillus is a scientific fact; all, with the same facilities, may see what others have seen. It is the one thing tangible, describable, known by its peculiarities among entities as readily as one individual is known from another. To doubt its existence in tuberculosis is to doubt the utility of scientific medical research, and to abandon further progress to the unstable dreams of theorists. The sputa of the phthisical contain these germs; the air they exhale is loaded with them or their spores, and their introduction into the system of animals will always produce tuberculosis, *while nothing else will.* These are not speculations, but demonstrable facts! Furthermore, clinical observations

go to prove conclusively that healthy individuals, living in an atmosphere contaminated by the phthisical, will contract this disease, and not any other which might be due to a lowered vitality, from being in close quarters and breathing a vitiated air. That there is yet much to be learned in regard to the tubercle-bacillus, there can be no doubt. Still, having made a wide breach in the walls that hemmed in the mystery of tuberculosis, it behooves us to press on to its complete solution.

And now, Mr. President and Fellows of the College, my remarks on the question at issue are, for the present, at an end; but I feel that I would be recreant to the cause I have espoused did I not avail myself of this opportunity to state that, in more than one instance, in articles recently published, the non-contagionists, it seems to me, have wilfully, unhesitatingly, and without warrant, perverted the language, even absolutely falsifying the statements, of authors they quote in support of their cause. That such reprehensible practices should be resorted to, for what must necessarily be but a momentary triumph, is of itself strong evidence of the vulnerability of their position, and requires no word of condemnation from me; nor would I think proper to notice it at this juncture, were it not to point out the necessity for all conscientious investigators to verify every and all citations by referring, wherever possible, to the original documents. And if my feeble efforts have, in the slightest degree, advanced the cause of truth and humanity, my labor has not been in vain. Now—

"Say as you think, and speak it from
your souls."
"What you do
Still betters what is done."

NOTES OF A CASE

OF

INFECTIOUS, SO-CALLED ULCERATIVE ENDOCARDITIS, AND OF A CASE OF ACUTE PERICARDITIS.

By

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AND

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[Read March 4, 1885.]

THE following case of endocarditis of the infectious or ulcerative variety is presented for your consideration, to-night, on account of its comparative rarity, and the extreme interest that is aroused by some striking features in its cause, its course, and its anatomical characters. The patient was under the care of Dr. Ludlow in the Presbyterian Hospital, and our obligations are due him for the privilege of this presentation. The following is an abstract of the ward notes of Dr. Hamaker.

CASE.—Infectious endocarditis secondary to acute rheumatism; intermitting fever; sweats; diarrhœa; pneumonia. Death. Proliferative endocarditis; micrococci; renal emboli.

W. J., white, æt. 30, baker, was admitted to the medical wards of the Presbyterian Hospital, October 3, 1884, suffering from rheuma-

tism, localized in the left knee and the right shoulder and elbow. He stated that he had been feeling badly for some time, losing flesh and strength. Previous to the present illness, he had had smallpox, syphilis, and gonorrhœa.

In addition to the joint pain, it was found he had a temperature of 104°, oppression behind the sternum, with pain aggravated by a slight cough. His face was pock-marked; he had the appearance of a drinking man, and was extremely anæmic. No cardiac murmur detected.

On October 10th, one week after admission, it was noted the cough and substernal distress had disappeared, the temperature had fallen to the normal, the joint pains were much relieved. The joints had not been swollen or red, but were painful. It was, at this time, thought worthy to note the occurrence of profuse sweats, both day and night.

Oct. 21. Fever and joint trouble recurred; sweating continues; occasional diarrhœa, soft systolic murmur noted at the apex.

25*th*. Nose bled profusely, requiring plugging of the right nostril.

Nov. 1. Pneumonia of lower lobe of right lung. Mitral and aortic murmur heard. No joint pains.

19*th*. Resolution of the pneumonic consolidation; return of joint symptoms.

Dec. 9. Joint symptoms increased in severity and became general. The fever has continued intermitting in type (*see* Chart), the sweating more marked, the pulse and respirations extremely rapid. Past two days, congestion of both lungs. Death at 6 P.M., to-day (9th), or about ten weeks after admission.

Autopsy, twenty-two hours after death.—Head and joints not examined. Bloodless appearance of face and extremities; emaciation of slight degree; marked rigor mortis; large ecchymoses on back of trunk and legs; pin-head size purpuric spots over trunk and extremities, anterior.

Thorax.—Lungs œdematous, and at bases, posterior, the seat of hypostatic congestion; pieces float on water; a few hemorrhagic infarcts are seen in the middle lobes.

Microscopical Examination.—The results of an inflammatory action are everywhere present. The interalveolar connective tissue,

especially in the immediate vicinity of the bloodvessels, is greatly increased, and contains a notable number of new cells, as well as quantities of dark pigment. The alveoli are generally distended, being partially filled with shed proliferated lining epithelium, while their walls are thickened.

The pleuræ were healthy in appearance. The pericardium was normal; its sac contained a small amount of serum.

Heart.—Slightly hypertrophied. Its exterior appeared normal; on section the walls were normal, and but slightly increased in thick-

FIG. 1.

Mitral valves, showing inflammatory outgrowths.

ness. In both ventricles, ante-mortem clots were seen extending far into their respective vessels. On removal of the clot from the left ventricle a portion of the inflammatory exudate was torn off, the fibrin having been intermingled with it. After this removal the auricular surface of each segment of the mitral valve was seen to

be fringed with grayish-white outgrowths. They were quite soft and varied in size from a bead to a cherry, and were irregular, warty, or roughened in appearance. A few outgrowths were also seen on the ventricular surface of the valve segments, and some were attached to the chorda tendineæ. (Fig. 1.[1]) No extension of the inflammatory process was seen on the endocardium of the auricular or ventricular

Fig. 2.

Tricuspid valve in longitudinal section. $\times$ 18 diam.

walls. On removal of the vegetations, a roughened appearance of the valve was observed, but no ulceration. The aortic valves were similarly encrusted with these vegetations, apparently springing from the corpora arantii. The pulmonary valves were normal. The aorta

[1] Three photographs and four micro-photographs, taken by Dr. Piersol, were exhibited. The photographs showed the outgrowths on the mitral, tricuspid, and aortic valves respectively. The micro-photographs showed sections of the valves and masses of micrococci, $\times$ 200 and $\times$ 700 respectively. The wood-cuts were cut from the photographs.

was healthy, save near its origin, where a few small spots of atheroma were detected; small aneurisms observed by others were absent.

Microscopical Examination.—The outgrowth examined was taken from the posterior curtain of the tricuspid valve. Its general appearances and arrangement, as seen in longitudinal section under low amplification, are accurately shown in the accompanying photo-micrograph. (Fig. 2.) The upper surface of the valve is covered with irregular, dense masses, forming a broken, but complete, investment. The under surface is generally free, some sections showing a few small areas of the same nature. By their intense and tenacious staining with methyl-blue, as well as by other characteristics, these masses

FIG. 3.

Masses of micrococci in the substance of the valve. × 200 diam.

appear composed of large zoöglœar tracts of micrococci. Examination under a power of 1000 diameters (Zeiss $\frac{1}{20}$) confirms this supposition, suitable parts of the masses being readily resolved into the individual cocci. These masses of micrococci apparently rest *upon* the endothelium; in places, however, they penetrate to the subjacent tissue by processes, which recall strongly the penetrating pegs of epithelioma. The micrococci are arranged in groups, varying greatly in size, and generally distinctly limited. In several places, the micrococci occur in the connective tissue layer of the valve, as cir-

cumscribed, well-defined, oval masses, as seen in the photograph. (Fig. 3.) The subendothelial tissue in many fields is filled with diffused micrococci; the cell proliferation in this layer, however, being pronounced but in few places. The bulk of the growth is due to a very marked proliferation of the elements of the central, connective tissue layer of the valve. The fibres are separated and crowded apart by numerous small round and oval cells. On the under surface of the valve, about one and a half millimetre from the edge, exists a large oval mass—slightly less than one millimetre in diameter—composed principally of micrococci, among and dividing which are delicate septa, in connection with the delicate capsule surrounding the entire mass. Embracing this mass, and extending along the valve to and beyond its edge, is a thrombus. The surfaces of this are invested by a distinct membrane. Throughout the thrombus, among the well-preserved blood-cells, minute masses of micrococci are to be seen.

Examination of the wall of the left ventricle shows the muscular tissue itself to be unchanged; in the intermuscular connective tissue, especially near the endocardium, however, there is an increase in cellular elements.

Abdominal Cavity.—Liver weighs four pounds ten ounces. Fatty and slightly cirrhotic. Gall-bladder and ducts normal. *Spleen* weighs one pound five ounces, soft and pulpy. Malpighian bodies very marked, capsule thick and opaque. Neither organ contained hemorrhagic infarcts. *Kidneys*, each weigh ten ounces. Both have the appearance of the large white kidney. In the left a small cyst is seen, and in the right a very large hemorrhagic infarct.

Microscopical Examination of the Kidney.—The intertubular connective tissue is everywhere increased, and contains great numbers of small cells, this being especially marked around the larger blood-vessels and Malpighian bodies. These latter present conspicuous changes. From a condition where the glomeruli are simply swollen, distending the capsule, with, perhaps, an increase of nuclei, all stages of glomerulitis may be found—the capsule disappearing in the surrounding mass of inflammatory cells; finally, the complete atrophy of the Malpighian body. The uriniferous tubules of the labyrinth are distended with swollen granular cells; frequently they are occu-

pied with plugs of degenerated epithelium, blood-cells, and granular débris. The bloodvessels are engorged; in places near the Malpighian bodies partially absorbed extravasations are to be noticed. The larger vessels show some thickening of their walls. In a few vessels, in transverse section, micrococci were distinctly seen entangled among the blood-cells and proliferated endothelium.

Remarks.—During the period of observation—viz., ten weeks—the sequence of events was, acute articular rheumatism, the symptoms of which ameliorated in one week; a period of uncertain convalescence characterized by profuse night sweats; a recurrence of the rheumatism after ten days, with coincident endocarditis; a second remission of the joint symptoms, of twenty days duration, during which a pneumonia ran its course to almost complete resolution; a third relapse of rheumatic symptoms, with aggravation of the endocardial mischief, terminating fatally.

The somewhat meagre clinical history above detailed represents to a fair degree the course of infectious endocarditis. The points of especial interest are worthy of more distinct mention, and are as follows:—

a. The cause of the disease in this case was undoubtedly rheumatism. There was no focus of infection in any portion of the body. This is contrary to the general rule, for the cases analyzed by Prof. Osler more frequently originated from other causes than rheumatism.

b. The occurrence of pneumonia in the course of the disease is worthy of remark. Frequently pneumonia occurs synchronously with the development of the cardiac inflammation, or even anterior to that process; so that the question has been discussed whether the pneumonia is not the primary infective process. Developing

in the middle period of the disease, our case would lead one to infer that it was probably an accidental complication, more liable to occur on account of the disturbances in the circulation; markedly more so, if the right heart, as in our case, was involved.

c. The usual characteristic symptoms were present to a high degree,—intermitting fever, sweats, diarrhœa, dyspnœa, and rapid pulse. The course of the fever was so distinct and is so instructive that it is well worth while to give it in full. It is characterized by marked intermittency. It is appended with the record of the pulse and respiration the last three weeks of the patient's illness.

Record of temperature.

	A. M.	P. M.	
Oct. 3,		104°	
" 4,	$102\frac{2}{5}^\circ$	$102\frac{4}{5}^\circ$	
" 5,	$102\frac{3}{5}^\circ$	$102\frac{4}{5}^\circ$	
" 6,	102°	$103\frac{3}{5}^\circ$	
" 7,	102°	$98\frac{2}{5}^\circ$	
" 8,	$99\frac{2}{5}^\circ$	$100\frac{1}{5}^\circ$	
" 9,	$97\frac{4}{5}^\circ$	$97\frac{3}{5}^\circ$	
" 10,	$96\frac{3}{5}^\circ$	$96\frac{4}{5}^\circ$	
" 11,	$96\frac{2}{5}^\circ$	99°	
" 12,	98°	98°	
" 13,	97°	$99\frac{3}{5}^\circ$	
" 14,	$98\frac{3}{5}^\circ$	$99\frac{2}{5}^\circ$	
" 15,	$99\frac{2}{5}^\circ$	101°	
" 16,	$98\frac{2}{5}^\circ$	$100\frac{1}{5}^\circ$	
" 17,	$97\frac{4}{5}^\circ$	$99\frac{2}{5}^\circ$	
" 18.	$98\frac{2}{5}^\circ$	$100\frac{3}{5}^\circ$	
" 19,	99°	$101\frac{3}{5}^\circ$	
" 20,	$101\frac{3}{5}^\circ$	$100\frac{3}{5}^\circ$	
" 21,	$102\frac{2}{5}^\circ$	$100\frac{4}{5}^\circ$	Endocarditis.
" 22,	$100\frac{1}{5}^\circ$	$100\frac{3}{5}^\circ$	
" 23,	100°	$100\frac{3}{5}^\circ$	
" 24,	$100\frac{2}{5}^\circ$	$102\frac{2}{5}^\circ$	
" 25,	$101\frac{1}{5}^\circ$	$100\frac{4}{5}^\circ$	
" 26,	$100\frac{3}{5}^\circ$	$101\frac{2}{5}^\circ$	

			A. M.			P. M.	
Oct. 27,			101$\frac{2}{5}$°			101$\frac{3}{5}$°	
" 28,			102$\frac{3}{5}$°			98$\frac{3}{5}$°	
" 29,			100$\frac{1}{5}$°			100$\frac{1}{5}$°	
" 30,			100$\frac{4}{5}$°			102$\frac{1}{5}$°	
" 31,			101$\frac{3}{5}$°			98$\frac{1}{5}$°	
Nov. 1,			99$\frac{4}{5}$°			102$\frac{2}{5}$°	Pneumonia.
" 2,			99$\frac{1}{5}$°			102°	
" 3,			99$\frac{1}{5}$°			100$\frac{1}{5}$°	
" 4,			102$\frac{3}{5}$°			102$\frac{4}{5}$°	
" 5,			102$\frac{3}{5}$°			103$\frac{3}{5}$°	
" 6,			103$\frac{1}{5}$°			103°	
" 7,			101$\frac{2}{5}$°			101$\frac{4}{5}$°	
" 8,			99$\frac{1}{5}$°			104°	
" 9,			103°			101$\frac{3}{5}$°	
" 10,			102°			104°	
" 11,			98$\frac{3}{5}$°			98$\frac{2}{5}$°	
" 12,			99$\frac{2}{5}$°			103$\frac{1}{5}$°	
" 13,			101$\frac{2}{5}$°			99$\frac{2}{5}$°	
" 14,			98$\frac{4}{5}$°			98$\frac{2}{5}$°	
" 15,			98$\frac{2}{5}$°			101$\frac{2}{5}$°	
" 16,			101$\frac{1}{5}$°			100$\frac{1}{5}$°	
" 17,			101$\frac{4}{5}$°			102$\frac{1}{5}$°	
" 18,			101$\frac{4}{5}$°			102$\frac{1}{5}$°	
" 19,			102°			103$\frac{3}{5}$°	
" 20,	Resp.	Pulse.	101$\frac{4}{5}$°	Resp.	Pulse.	98$\frac{2}{5}$°	
" 21,	28	124	102$\frac{1}{5}$°	32	140	103$\frac{4}{5}$°	
" 22,	25	100	96$\frac{2}{5}$°	30	112	99$\frac{1}{5}$°	
" 23,	28	124	102°	30	117	101$\frac{3}{5}$°	
" 24,	32	112	101$\frac{2}{5}$°	28	115	103$\frac{2}{5}$°	
" 25,	28	108	101$\frac{1}{5}$°	20	100	101$\frac{3}{5}$°	
" 26,	28	108	101$\frac{1}{5}$°	28	148	102$\frac{3}{5}$°	
" 27,	24	100	100$\frac{4}{5}$°	26	106	102$\frac{4}{5}$°	
" 28,	30	121	$100^{4\circ}$	34	112	102$\frac{3}{5}$°	
" 29,	30	112	101$\frac{4}{5}$°	40	118	102°	
" 30,	28	100	100$\frac{3}{5}$°	38	156	103$\frac{3}{5}$°	
Dec. 1,	28	110	99$\frac{4}{5}$°	24	114	98$\frac{3}{5}$°	
" 2,	28	108	99$\frac{1}{5}$°	44	132	102$\frac{1}{5}$°	
" 3,	32	120	102°	34	140	103$\frac{2}{5}$°	
" 4,	30	120	101$\frac{4}{5}$°	30	126	103$\frac{3}{5}$°	
" 5,	21	106	100$\frac{4}{5}$°	32	144	103$\frac{3}{5}$°	
" 6,	30	130	103°	38	168	103$\frac{3}{5}$°	
" 7,	30	135	103$\frac{2}{5}$°	48	150	103$\frac{3}{5}$°	
" 8,	42	150	104°	44	138	103$\frac{3}{5}$°	
" 9,	36	120	$103^{3\circ}$				

d. The appearance of the inflammatory lesions is remarkable. It was found on careful examination that no actual ulceration took place, but that the process was proliferative in nature, it being represented by the outgrowths on the valves.

Lancereaux described some cases which presented this appearance. Attention should be called to the occurrence of micrococci in the inflammatory products, not only of the endocardium, but in the glomeruli of the kidneys as well. The occurrence of the inflammation in the right ventricle is also worthy of remark. The absence of inflammation in this cavity has been said to be due to the absence of oxygenated blood. This case, as well as others of a like nature, certainly shows that a septic inflammation, to say the least, can take place in this locality.

Acute Pericarditis.—The clinical interest, otherwise attached to the next case, is somewhat detracted from by the short period it was under observation. It was, no doubt, originally a case of uræmia secondary to interstitial nephritis. The pericarditis developed in the course of the uræmia superinduced by exposure incident to removal to the hospital. One might consider the cerebral symptoms attributed to the uræmia, due to the pericarditis. The subnormal or normal temperature, the absence of physical signs of pericarditis on admission, and the absence of sufficient lesions in the pericardium to cause death (excessive fluid, etc.), leads one to infer that the uræmia preceded the local inflammation, and caused the death of our patient. In addition to the above points of interest, the age of the patient, and the absence apparently of sufficient cause for so grave organic disease of the kidneys are worthy of attention.

CASE.—J. H., female, æt. 35, white, servant, admitted to hospital under care of Dr. Ludlow, December 23, 1884.

No knowledge of her previous history could be obtained, save that she had had some œdema of face and ankles, and an obstinate vomiting for a few days.

When admitted she was seen to be anæmic, but not emaciated, and that her face was puffy and her ankles swollen. The vomiting of a clear fluid continued without nausea. She did not complain of headache, but was dull, and slightly delirious. Heart normal; lungs congested posteriorly. Urine, spec. grav. 1004, highly albuminous with small granular casts; quantity in twenty-four hours, thirty-eight ounces.

Dec. 24.—Condition same. In the evening, a faint, double friction sound heard over the base of the heart.

25*th.*—Friction sound louder, and more distinct. Fremitus. No cardiac pain.

26*th.*—Roughened character to friction sounds. Vomiting continues. Temperature 98° P.M. No rise above since admission. Stupor.

27*th.*—Convulsions. Coma. Death.

Autopsy, nineteen hours after death.—Rigor mortis marked; œdema of legs and face. Pallid countenance. *Heart*, fourteen ounces, hypertrophied. Parietal and visceral layer of pericardium strewn with flakes of recent lymph, and on heart small villous outgrowths. No blood effusion or ecchymoses. No increase of serum. On opening heart, valves found normal, save small inflammatory patch on one leaflet of mitral valve. Atheroma of small amount first six inches of aorta. *Lungs* slightly œdematous. *Liver* congested and fatty; gall-bladder full of black bile; ducts patent. *Stomach* congested and mamillated. *Duodenum* congested and stained with bile. *Kidneys* weigh two and two and one-half ounces, respectively; capsules thickened and opaque; removed with difficulty. Cortical portion reduced in size and irregular; about junction of cortical and medullary portion, white striæ parallel to the medullary rays, looking not unlike deposits of calcareous matter, but which did not respond to the usual tests.

8

Microscopical Examination of the Kidney.—Very evident changes apparent throughout the organ. The capsule is thickened and adherent, its inner layers being infiltrated with small round cells. The connective tissue is increased, and its elements are actively engaged in proliferation, some fields being almost continuous tracts of closely crowded small round cells. The Malpighian bodies are generally in some stage of functional decline—from the beginning thickening of the capsule to final atrophy, many showing a complete amyloid degeneration. The cells of the uriniferous tubules are swollen, granular, or fatty. Many tubules contain waxy casts, and certain of the straight collecting tubules are impacted with granular fatty masses of desquamated cells—these tubules probably accounting for the whitish striæ noticed macroscopically. Many of the larger collecting tubules are entirely destitute of lining cells. The bloodvessels have greatly thickened walls, the increase taking place in their median and adventitious coats.

Microscopical Examination of Pericardium.—Sections of a patch from the parietal pericardium show an irregular mass composed chiefly of small round cells, embedded in a partially fibrillated matrix, the mass assuming the form, on its free surface, of irregular papillary processes. The individual cells are but poorly defined, and central irregular tracts are undergoing degeneration. The subendothelial tissue is infiltrated with small cells, proliferation of the elements of the tissue being pronounced. Micrococci were not distinguished in this mass.

[After the reading of the preceding paper:—]

Dr. Da Costa stated that he had listened with a great deal of interest to the report of the case of ulcerative endocarditis. It is certainly not a common affection in this country. Contrary to the usual opinion, in the instances which he had seen, there was mostly an association with acute rheumatism. The combination with pneumonia, on which some excellent observers have laid great stress, he had not often met with.

Dr. Musser said: I should like to make a remark in reference to the statistics in regard to rheumatism and pneumonia in ulcerative endocarditis. As I stated, these statistics are those of Dr. Osler, found in his article on infectious or ulcerative endocarditis. (*Transactions Internat. Med. Congress*, 1881.) I believe that he collected one hundred and fifty or sixty cases, in only one-third of which was rheumatism present. In the other cases, there was pneumonia or some other primary source of infection.

He has since told me that he has found this form of ulcerative endocarditis to be excessively frequent in pneumonia occurring as a complication. I am not able to give exactly his figures, but I think that in one hundred and three cases of pneumonia which he had examined, he had found it sixteen times.

A CASE

OF

DERMOID OR PILIFEROUS TUMOR; WITH CURE BY SPONTANEOUS OPENING INTO THE INTESTINE.

BY

WALTER F. ATLEE, M.D.

[Read March 4, 1885.]

M. C., residing at 1014 Carpenter St., in this city, came to me in May, 1884, on account of a lump she had that day discovered in the lower part of the belly. She was 32 years of age, of medium size, with the appearance of good health. Her menses always came regularly every four weeks; never were profuse; and, as a general rule, lasted but two days.

This lump appeared to be egg-shaped, the long diameter in the direction of the spinal column, in size perhaps six inches in its greatest length, and placed rather above the womb, and somewhat to the left of the median line. She complained of pain and of obstruction to the passage of the stool. For this condition the use of a pill composed of opium and of the compound extract of colocynth was advised. A few days afterwards, on account of complaint of difficulty in passing the urine, a drink of bitartrate of potassa was recommended for use. In July, on account of troubles of digestion, bismuth and strychnine were prescribed, to be used before meals, and pills of carbonate of iron to be taken after. Early in September pills of ergotine were ordered, and their use was continued again in October and in November and December. During this time the lump appeared to be growing rather smaller, and it was certainly becoming

softer. On the 11th of November a considerable quantity of matter like thick gruel, white, and with no odor, came away from the bowel. This, under the microscope, was seen to be composed of the exfoliated epithelial cells and the secretion from sebaceous glands. This continued to come from time to time, and as it did, the lump became smaller. On the 10th of January hair began to come, some of it long and very black, and the most of it of a light brown, and but four or five inches in length.[1] On the 16th of January the matters that came away were very small in quantity, greenish in color, and of a very bad odor. Since that time there has been nothing of notice in the discharges from the bowels, except twice a slight quantity of blood. There is now no sign of the tumor.

These dermoid or piliferous tumors are interesting, surgically, from the obscurity they throw over diagnosis, and in the complications they occasion. In the case just related, the tumor was believed to be a uterine fibroid for several months; in fact, until it was observed to become quite soft, from hard, as it had always been before. In a pathological point of view their interest is very great indeed, and also in one of comparative physiology. By far the larger number of dermoid cysts—indeed, the immense majority of them—are ovarian; but no matter in what part of the body they may be found, or what may be the sex of the patient, the doctrine of "the continuous development of tissues out of one another," as Virchow calls it, will suffice to account for the growth of all ordinary dermoid or piliferous tumors. Inherent in the tissues of the body is a peculiar formative and reproductive power, and it operates in the production of these strange tumors, as it does in the large number of multiform morbid growths which spring up everywhere under circumstances impossible for us to explain.

[1] A considerable quantity of this hair was shown to the College.

In ovarian morbid growths the kind of tumor that is formed depends upon the strength of the formative impulse. In ordinary cases, the force of formative power goes no further than the production of cyst-walls with a secreting endothelium which pours out fluid contents. In some cases the cell growth is enormous, and yet there is no disposition to organization, and piliferous excrescences, cancerous and colloid masses, show themselves. In others, again, imperfect attempts at organization are seen, as in those called dermoid or piliferous, on account of what is most usually found in them.

The origin of all these cysts is traced, indirectly by way of exclusion, and directly by way of observation, to the development of the ovisacs or Graaffian vesicles; and, when we consider what these ovisacs are, we need never be surprised at the contents of these cysts, and at the abundance, the nature, and the variety of these contents. When skin, hair, teeth, and so on, are met with, it is quite in accordance with known facts in comparative physiology to look upon tumors containing them as imperfect attempts at organization arising from the powerful germinative aptitude of the ovary. The formation of imperfect tissues is reached without ever going so far as the formation of an organ, let alone that of an organism. Such formations may be looked upon as examples of parthenogenesis, as imperfect vestiges in the higher animals of a regular physiological act in some of the lower ones.

[After the reading of the preceding paper:—]

Dr. Formad said: A few weeks ago I met with an ovarian cyst of quite unusual dimensions. The woman from whom the tumor was removed was 35 years old, and had led an irregular life. The tumor was large, and suggestive of an ovarian cyst. On opening the abdomen, it was found attached by a very long pedicle to the left ovary, and directly starting from it. When removed, the tumor weighed fifteen and one-half pounds. It was incised, and twelve pounds of sebaceous material escaped. The autopsy was performed while the body was still warm. The sebaceous material at once congealed, and in the course of an hour presented the appearance of tallow. The cyst itself was multilocular, having five or six divisions. These loculi contained a number of long growing hairs, which sprang directly from the inner surface. There were also a number of cartilaginous and bony masses, but no distinct formation of teeth was seen. Subsequently inquiring into the past history, I learned that the tumor had been growing from the time she was five years old. I mention this as an unusual case, and will some time exhibit the specimen.

NOTE ON A SPECIMEN

OF

ENORMOUSLY HYPERTROPHIED HEART, WEIGHING FORTY-EIGHT OUNCES.

PRESENTED BY

CHARLES W. DULLES, M.D.,

SURGEON TO THE OUT-PATIENT DEPARTMENTS OF THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA AND OF THE PRESBYTERIAN HOSPITAL.

[Read March 4, 1885.]

CASES of essential hypertrophy of the heart are not rare; but cases in which the hypertrophy has been as great as in the specimen I desire to place in the Mutter Museum of the College, are very rare indeed.

The history of the case from which I obtained this specimen is briefly as follows.

J. D., a young man 18 years old, with an unusually large frame for his age, had long been a sufferer with rheumatism, when he came under my care, in 1882, in an acute attack. At the time I first saw him, he was suffering with great dyspnœa, and on examination I found he had an enormously dilated and hypertrophied heart, with a strong, harsh, mitral systolic murmur. His urine was orange-brown, containing bile pigments in considerable quantity, a great excess of urates, a deficiency of phosphates, and no albumen or sugar. In the sediment there were no evidences of renal disease. His pulse was 120, and tumultuous, and there was, of course, a strong apex im-

pulse. I cleared his bowels out with one grain doses of calomel, given hourly, and then gave him some morphia and spirits of chloroform to allay his pain. The regular treatment consisted of the use of digitalis with iron and strychnia, to steady and slow the heart. This, however, did but little good, and entirely failed to reduce the rapidity of the pulse. At one time I tried the effect of veratrum viride; but this was absolutely useless. The only good result I got was from the administration of twenty grain doses of the iodide of potassium thrice daily. This led to the disappearance of the murmur, the almost entire disappearance of a cough which had troubled the patient, to the clearing up of his urine, to his sleeping well and eating well, and to his being able to get about again.

This amelioration lasted for six weeks, when he got worse again, and had a bad attack of gastric disturbance, dyspnœa, and some cyanosis. Soon fluid began to accumulate in his abdomen and thorax, as well as under the skin of his extremities. He now had a most beautiful water-hammer pulse in both femoral arteries, just below Poupart's ligament. Three weeks later he had a similar exacerbation of his symptoms, and three weeks afterwards another. He now had considerable albumen and a number of hyaline tube casts in his urine.

One night, after he had been under my care for four months and a half, I was called to him, and found him, as I had done on other occasions, laboring with intense dyspnœa and great cyanosis. I gave him, as I had done before, a hypodermic injection of hydrochlorate of pilocarpine. As I had found one-fifth of a grain apparently too small a dose before, I now administered one-third of a grain. This brought on free sweating, and the discharge of about two ounces of saliva. His pulse came down from 140 to 100 in half an hour, and his dyspnœa was moderated. But he continued restless, the dyspnœa returned, his respiration varied from 35 to 60 in the minute, his cyanosis increased, his bowels moved involuntarily, his lungs seemed to fill up with œdema, and he soon died asphyxiated.

At the autopsy, in which Dr. Formad and Dr. J H. Musser kindly assisted me, the abdomen was found to contain about two quarts of clear serum, and each side of the thorax about as much. There was general œdema of the feet and legs. The lungs were

compressed, but not diseased, though they were œdematous. The liver was somewhat fatty. The spleen was hard and tough, but of normal size. The kidneys were large, congenitally lobulated, and in a state of cyanotic hypertrophy. The heart was enormously hypertrophied, weighing, *after all the adherent parts were thoroughly removed, forty-eight ounces.* The pericardium was everywhere firmly adherent to the heart, and could not be separated from it, except with the knife. There was, therefore, no pericardial cavity whatever. After opening and washing out the clots from the heart, the weight was forty ounces. All the valves were healthy, and seemed to be competent. The mitral orifice was enormous, measuring two and a half inches across. The muscular walls of the heart were symmetrically hypertrophied. The whole organ was about the size and had about the appearance of that of a bullock.

The best estimate as to the average weights of various organs of the body, which I know of, are those of the late Professor Reid, of Edinburgh. In his table,[1] the weight of the heart, between the ages of 16 and 20 years, is under seven ounces (six ounces and thirteen and four-tenths drachms). When this weight is compared with the total of forty-eight ounces (gross), in the present case, it will be seen how great was the excess here.

[1] Tables of the Weights of some of the most important Organs of the Body at different Periods of Life, by John Reid, M.D., etc., Professor of Anatomy and Medicine in the University of St. Andrews. London and Edinburgh Med. Journ., April, 1843, pp. 295–323.

[After the reading of the preceding paper:—]

The PRESIDENT asked what was the supposed origin of the trouble.

Dr. DULLES said: There are two things to account for it. One is heredity. The other is rheumatism inducing primarily pericarditis. There is a sister much younger, who has the same condition of hypertrophy, and is, I think, going the same way.

PHOTOGRAPHY OF THE LARYNX.

By
THOMAS R. FRENCH, M.D.,
BROOKLYN, N. Y.

[Read by invitation April 1, 1885.]

I AM very sensible of the high compliment paid to me by the passage of the resolution at the last stated meeting of the College, inviting me to address you this evening. It is with great pleasure that I avail myself of the opportunity offered, to show this distinguished body some of the photographs which I have taken of the living larynx, and to explain the manner in which the larynx can be photographed.

Twenty-eight years ago, Professor Czermak, of Pesth, perfected the art of laryngoscopy. As soon as he found that, by the aid of a strong light thrown into the back of the throat, and a small mirror held in the fauces, the interior of the larynx in the living subject could be clearly revealed, he made an attempt to get a photograph of the parts thus exposed to the eye. The result was, however, very unsatisfactory; but large allowance must be made for the fact that in those days instantaneous photography was a thing unknown. Since then ineffectual attempts have been made by many laryngoscopists.

Two years ago Mr. Lennox Browne, of London, succeeded in obtaining three or four good photographs of

one larynx. When exhibiting those photographs at a meeting of the British Medical Association, he remarked that he did not anticipate that photography of the larynx could be carried beyond the boundary of physiology, and to expect photographs from life of pathological conditions was plainly unreasonable.

Three years ago, with the assistance of a gentleman possessing rare scientific attainments, and who is a skilled amateur photographer, Mr. George B. Brainerd, of Brooklyn, I made my first attempt to photograph the larynx, Mr. Brainerd acting as subject. Our first results were not satisfactory, but we continued our experiments till last spring, when they were brought to a successful termination. Perfect photographs can now be obtained of any larynx, whether in a healthy or diseased condition, with ease and without assistance.

There are many ways in which I believe that photography of the larynx will prove of value.

1. By enabling us to obtain exact reproductions of the laryngeal image. This will assist us in becoming familiar with the difference in size, shape, and position of the various structures of the larynx.

2. In the study of the physiology of the larynx.

3. In the study of the pathology of the larynx.

4. By enabling us to furnish truthful illustrations for published reports of cases. Heretofore it has been necessary to use drawings to illustrate cases, but drawings are at best only approximations to the truth, which vary according to the skill of the draughtsman, and his familiarity with the parts which he attempts to represent.

5. By enabling us to show types of the larynx, both normal and diseased, in text-books.

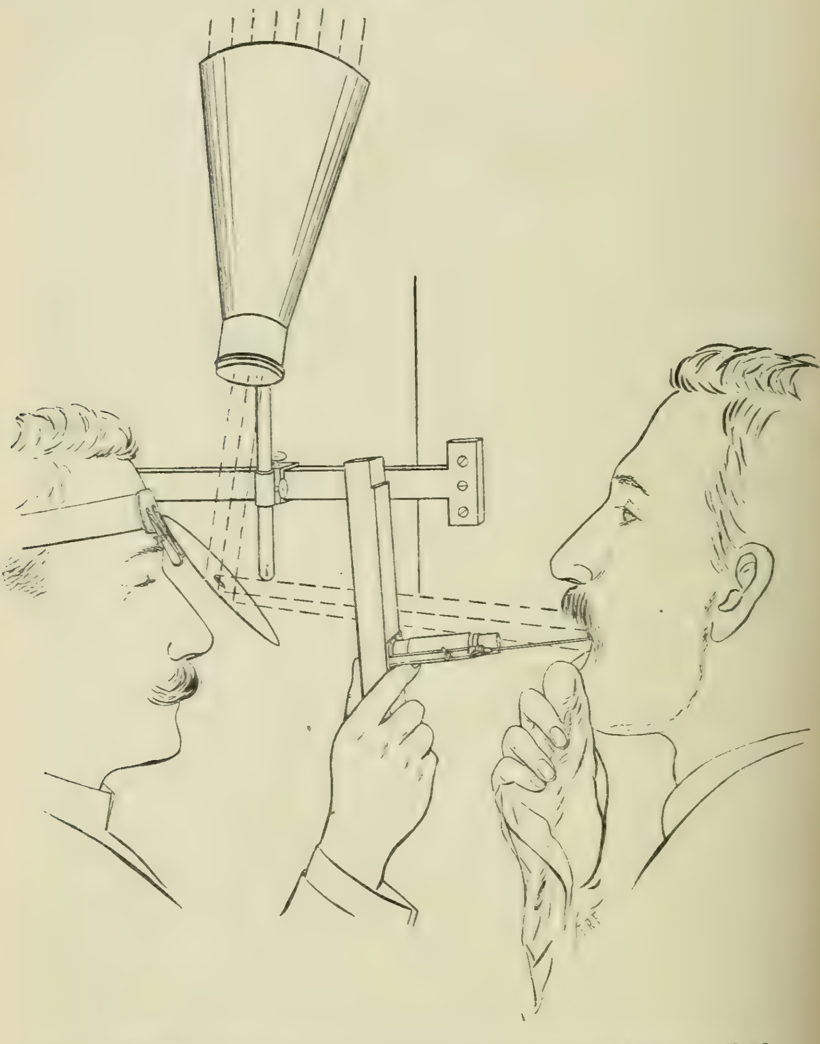
6. By enabling us to show photographs of the larynx with the lantern, in class-room instruction.

7. In reproducing the exact laryngeal images for the records of patients' histories. In this way we can study the existing conditions at leisure, and therefore more thoroughly.

We can introduce a speculum into the nasal passage, or into the external auditory canal, or into the vagina or rectum, and study the parts disclosed until the image is fixed upon our minds. The examination of the larynx is, however, conducted in a very different way. Here it is necessary to introduce a small mirror into the fauces, which, as a rule, will not tolerate it for more than a few seconds at a time. This necessitates frequent and repeated introductions of the mirror, in order to become familiar, not only with the picture as a whole, but with the relation and condition of the parts. In the great majority of cases, therefore, we acquaint ourselves with the laryngeal image by a succession of glances. Of course, the more practice one has, the more he will see of the larynx at a glance; but even the most skilful laryngoscopists cannot always appreciate the detail in a laryngeal image as well as if they were able to look steadily at it for a few minutes. I have made very careful laryngoscopic examinations of many subjects who were very tolerant of the mirror, and yet, after I had taken photographs of their larynges, I have found in them matters of detail which had entirely escaped my notice, and matters of detail may affect the prognosis and treatment, and I may say the diagnosis also, for in nervous affections it is at times very difficult, if not impossible, to determine the exact relative positions of the various structures, and yet it is only by a

perfect appreciation of these that an accurate diagnosis can be made. Photographs have already given me

Fig. 1.



valuable assistance in determining the exact seat of the paralysis.

I propose now to show upon the screen a few of the photographs which I have taken of the larynges of some of my patients. Before doing so, however, allow me to explain, with the aid of the photograph which is now being displayed upon the screen (Fig. 1 is a sketch of the photograph), the manner in which the larynx is photographed.

This picture shows a laryngoscopist in the act of photographing the larynx. In the window is an instrument by means of which the sun's rays are concentrated. It consists of a hollow truncated cone of metal, ten inches long, in the large end of which is a double convex lens. This has a focus of thirteen inches. In the small end is a plano-concave lens, which, when placed an inch or so inside of the point of focus of the large lens, intercepts the converging rays, and makes them parallel, as you can see. In this way we obtain a powerful beam of light, the heat from which is scarcely perceptible. The beam of light is caught upon the reflector, held upon the head of the examiner, and reflected into the fauces of the subject.

In the examiner's right hand is seen the camera. This is about ten inches long, one inch and a half wide, and an inch in thickness. Attached to its front face is a telescopic tube containing the lens. The throat mirror cannot be seen, because it is in the fauces of the subject. It is attached to a shank, which slides into an arm projecting from the front of the camera. Within the camera is a drop shutter. This is governed by a spring which is under the control of the forefinger. A holder, containing a plate, is inserted through a door on the back of the camera. The plate is so arranged that by sliding it up and down, five photographs can be taken upon it.

Now, when the examiner sees with his left eye, through the opening in the reflector, the image of the various structures of the larynx, in the throat mirror, in the position in which he wishes to photograph them, he presses upon the spring with his index finger, the shutter falls, and the exposure is made. In this way five photographs can be taken in from three to five minutes. In this city, several weeks ago, I took ninety photographs in ninety minutes.

The first five photographs are taken to find the focus of the larynx of that subject, and of those five, one or two will be in focus. The focus being ascertained, we can take as many good photographs of that larynx as are desired. Now let us examine some of the photographs which I have taken of the larynx in this way.

I feel certain that you will all recognize this photograph as one of the larynx in respiration. Every larynx, as seen with the laryngoscope, differs from every other larynx as every face differs from every other face. The features, or structures, are the same; but their size, shape, and relative position, vary somewhat. There is an effect shown in photographs of the larynx which cannot be so well shown in drawings; that is *depth.* In order that we may compare a photograph with a drawing, I have had a transparency made of a woodcut taken from Professor Türck's work on *Diseases of the Larynx.* It is probably as good a woodcut of the larynx as any which has been published. It is the conventional drawing used in books and reports to represent the laryngeal image. Of course, it is brighter than the photograph, for the reasons that it is not surrounded by dense tissue as in the photograph, and it is made of fine lines which allow the light to pass between them. You will observe that in this picture the cavity of the larynx looks shallow; it lacks depth. The distance between the anterior wall of the trachea and the top of the epiglottis is not as great as between the same parts in the photograph. The picture looks flat. You are now better able to appreciate that the photograph, which has again been thrown upon the screen, shows perspective, shows foreshortening, shows depth, better than it can be shown in drawings.

Seven photographs were then exhibited, to show the position of the various structures, in different larynges, in respiration and phonation.

This photograph (Fig. 2[1]) was taken of the larynx of a lady while singing a tone in the soprano voice. The vocal bands are seen to be short. They are not as short as they appear to be, for the epiglottis overhangs the larynx somewhat, and cuts off a view of a part

[1] The illustrations, representing the laryngeal image, are reproductions of drawings made from the photographs, by Mr. George C. Wright, of New York.

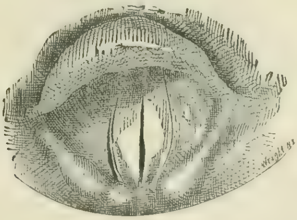
of them, but they are really quite short. Short vocal bands in the female indicate, as a rule, that the subject has a soprano voice.

This (Fig. 3) was taken of the larynx of a professional contralto singer, while singing the note E, treble clef, fourth space. The vocal bands are long, and long vocal bands in the female indicate, as a rule, that the subject has a contralto voice. The chink of the glottis is seen to be open widest in its posterior part. It is my belief

Fig. 2.

that the position of the vocal bands, in different larynges, in the production of the same tone, in the same class of voice, almost always differs. Sometimes the difference is but slight, though it may be marked. The difference is apt to be more marked in contralto and bass larynges than in soprano and tenor. In the larynges of other subjects possessing contralto voices, while singing the same note as

Fig. 3.

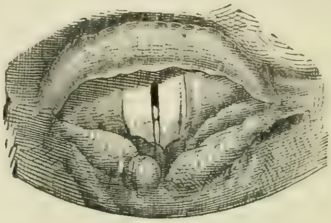


was being sung when this photograph was taken, we may find that the chink of the glottis, instead of having the shape as seen in this picture, will have a linear or an elliptical shape.

It is easier to compare photographs of the larynx than it is to compare living larynges. I have made careful comparisons of photographs of the larynges of many trained singers while singing the

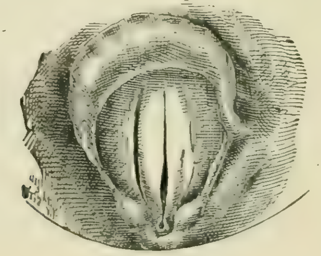
same note, and in the same class of voice, and have always found some difference in the position of the vocal bands. In some instances the difference is very great. The proof is therefore clear that the same note, sung by different individuals possessing the same class of voice, may be produced by different mechanisms.

Fig. 4.



This photograph (Fig. 4) was taken of the larynx of a gentleman while singing a tone in the tenor voice. Unfortunately, at the time that the photograph was taken, he was suffering from an attack of acute laryngitis, which accounts for the presence of mucus in the chink of the glottis, as well as upon one arytenoid cartilage. The vocal bands are seen to be short, and when we find short vocal bands

Fig. 5.



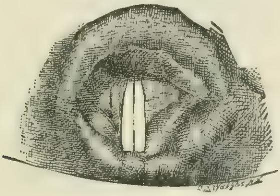
in the male, we can feel quite certain that the subject has a tenor voice.

This (Fig. 5) is a photograph of the larynx of a bass singer while singing the note B, bass clef. The vocal bands are seen to be long. They are usually long in subjects having bass voices. In this picture we see that the vocal bands are in contact at their posterior

insertions, leaving an opening in front, which is widest just in front of the point of contact, and a very small opening behind. The position of the vocal bands as seen in this larynx does not agree with any description which I have seen, of the mechanism in the production of low tones in the bass voice. In larynges of other subjects having bass voices, while singing the same tone, the chink may have an elliptical shape, and be open in its entire length.

Several photographs showing the position of the vocal bands in the production of tones in the falsetto and so-called head registers were then exhibited. In the larynges of subjects singing tones in the falsetto register, the positions of the vocal bands differed markedly. In one the chink of the glottis was linear in shape, and was open in its entire length. In another the chink was open only in front; while in the third, the vocal bands were in contact at their posterior insertions, leaving a slit in front, and a small opening behind.

Fig. 6.

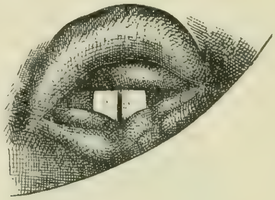


The lecturer, while speaking of the falsetto register, said: The position of the vocal bands varies in the falsetto as well as in other registers. It is the opinion of some writers that the chink of the glottis is always open widest in front. Others believe that the chink is open only in the middle; while others still assert that it is always open in its entire length. The truth is that the mechanisms vary, and this is proven by the photographs which you have just seen. The variation is, in my opinion, due largely to the difference in size and shape of the larynx, as well as in the length, breadth, and thickness of the vocal bands.

A young lady, æt. 16, the daughter of a popular singing teacher living in Brooklyn, came to my office one day last week to have me examine her throat, and hear her voice. To my great surprise, I

found that she possessed the extraordinary compass of four octaves, and could sing as high as C above the high C. Dr. Cohen, in his excellent little book, *The Throat and the Voice*, tells us that La Bastardella could sing this note, and that for its production the vocal bands must vibrate 2100 times in a second. I was fortunate enough to secure a photograph of the young lady's larynx while she was singing her highest note. This is it (Fig. 6). You will observe that the vocal bands are very narrow, quite long, and in very close apposition. Of course, they were being subjected to the highest degree of tension. I was also successful in getting a photograph of her larynx while she was singing her lowest note, C bass clef, second space (Fig. 7). Here you see that the vocal bands are very short,

Fig. 7.



very broad, and not in close apposition. They become longer and narrower, and are brought into closer apposition as the voice ascends the scale.

The next photograph is of the larynx of a child seven years old. I took it to prove that even the larynges of children can be photographed.

Thirteen photographs, showing pathological conditions, were then exhibited and explained. Two photographs of the posterior nares, the seat of hypertrophic catarrh, finished the exhibition.

A CRITICISM OF DR. LEEDS'S PAPER ON "THE COMPOSITION AND METHODS OF ANALYSIS OF HUMAN MILK."

By

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[Read May 6, 1885.]

DENIAL and reiteration prove nothing. I shall, therefore, endeavor to restrain myself from the use of either of them in my effort to compass my present purpose, which is to persuade the profession of the truth of what I advanced in regard to the composition of human milk, in a paper read before the Philadelphia County Medical Society, about a year ago.[1] I brought forward then certain arguments which seemed to me to prove conclusively that human milk contains only about one per cent. of casein. Those arguments it is not necessary for me to repeat, especially as I have nothing new to advance, and I must therefore refer anyone to whom the subject is new, but who feels an interest in it, to my former paper. At present I wish merely to criticise the results arrived at by Dr. Leeds and detailed in a paper read before the College.[2]

[1] Proceedings of the Philadelphia County Medical Society, vol. vi., 1883–4.

[2] Transactions of the College of Physicians of Philadelphia, 3d Series, vol. vii., 1884.

In such a criticism I shall not, of course, be restrained by a false delicacy from using all legitimate means to show why we should decline to accept Dr. Leeds's conclusions. The question in dispute is, how much casein human milk contains, and particularly what is its quantity relatively with that contained in cows' milk. Upon the decision of this question must depend the lives of a very large number of infants, for the conclusion at which we arrive as to the relative composition of the two kinds of milk must necessarily decide how we will use cows' milk as a food for young infants.

Dr. Leeds, in his paper, proceeds first to criticise previous methods of analysis and then goes on to describe the method used in obtaining his own results. I shall, therefore, begin by taking up his strictures upon my method of analysis, and then will discuss his own conclusions. In order that his criticism of the method I suggested may be understood, I shall have to give a brief description of my process as originally described. This was first published in the *Philadelphia Medical Times*, July 1, 1882, the paper having been read at the Philadelphia County Medical Society, the previous February.

To carry out the method, 15 c.c. of milk are required. Discharge 5 c.c. of milk from a pipette into a platinum dish and weigh; dry over a water-bath at 100° C. until the weight becomes constant, then incinerate, beginning with moderate heat and finally using the blast flame. This gives the water, total solids, and ash. Discharge 10 c.c. of milk in another dish and weigh, taking care that the weight be exactly double the first amount; pour this into a high narrow 100 c.c. graduated bottle and add 20 c.c. of distilled water, this being used to

wash all the milk from the dish. To this add 20 c.c. of ether and tightly stopper the bottle and agitate for five minutes, then add 20 c.c. of alcohol and again agitate for five minutes and let the bottle stand until the fluid separates into two layers. Draw off the upper layer, of ether containing fat, as nearly as can be done without taking any of the lower layer; pour in 5 c.c. of ether to mix with what fat is left, and draw off. Thus 5 c.c. of ether must be poured on and drawn off five times. The ethereal solution being dried over warm, and finally boiling water, gives the amount of fat. There is now left in the bottle the sugar, casein, and salts; this must be washed into a dish (best platinum) and dried over the water-bath. The residue is treated with boiling water and then allowed to stand; the casein sinks to the bottom and the sugar remains in solution. The clear solution is poured off and the residue again dried and treated with boiling water, and what is thus obtained added to that already had. This must be repeated until it is found that when boiling water is poured upon the dried sugar, complete solution takes place, no flocculi being seen in it. The casein residue is then, after being thoroughly dried, treated once or twice with boiling water to wash out any sugar that may have been left in it, care being taken that no casein is poured off. This sugar is added to that previously obtained, and the two residues are dried and incinerated, the loss in burning giving the weights of the casein and sugar respectively.

In Dr. Leeds's criticism of my method he finds fault, first, with the use of a pipette in measuring out the portions of milk to be used in analysis, saying that "the milk leaves minute particles upon its walls, and the alteration in composition thus produced is the greater,

the more extensive the wetted surfaces of the measuring vessel." This objection, although theoretically correct, is trivial, and the possible error introduced is not nearly so great as that likely to result from his own method of pouring out at a guess 5 c.c. of fluid, and thus have to weigh several times before getting sufficiently near the desired quantity to be able to proceed with the analysis. This is particularly the case when the 10 c.c. for the second portion of the analysis comes to be weighed, for it must weigh exactly twice that first used to make the analysis at all accurate. He says, in speaking of the first portion of my method, by which are obtained the water, total solids, and ash—"evaporation to dryness in a water-bath to constant weight, is tedious, usually requiring three hours, and is neither so accurate nor so expeditious as the method of coagulation with alcohol," and then after giving a comparison of the results obtained by trial of the two methods says—"in other words, at the expiration of seven hours, I had obtained the same constant weight as I had found by the method of coagulation at the expiration of one and one-half hours." Now he comes to this erroneous conclusion because he makes a mistake in his arithmetic with regard to the results obtained by his experimental trial. The actual fact is, that the addition of alcohol is an improvement in that portion of the analysis, although it expedites the last portion of the drying only very slightly, and does not prevent the formation of a skin upon the surface, as Dr. Leeds states. His mistake is as follows: he says, "to 5.1195 grms. milk added 3 c.c. alcohol, evaporated to dryness on water-bath . . . and then to constant weight in air-bath at 105°. Loss of weight 0.699 grm. or 13.56 per cent." This last sentence

contains two mistakes; it should have been—total solids 0.699 grm. or 13.65 per cent. This mistake in the percentage of the total solids is evidently not a mere typographical error, for upon it is based his conclusion that the addition of alcohol so very much expedites the drying. He then proceeds to take a second sample and dry it without the addition of alcohol; he drives off moisture until the total solids left amounted at the end of seven hours to 13.56 per cent. If he had dried the first sample with which he tested his own method to the same degree as he did the latter, he would have found that it took very little less time, for it is in driving off the last portions of moisture that the process is tedious. The sample dried by his own method was not then, as the figures when corrected show, brought to constant weight, but still contained some moisture, and thus his mistaken conclusion that the addition of alcohol so much expedites the process.

With regard to the extraction of fat, Dr. Leeds says, "When water is present, ether will extract not only fat but substances soluble in water. This was probably the case in the present instance, and experiment confirmed the conjecture. After distilling off the impure ether, drying the fat to constant weight at 105° and weighing, the fat thus obtained was redissolved in absolute ether. In every trial a residue was left behind. This residue dissolved readily in water. It proved to be milk-sugar." I have often tried redissolving the fat residues in absolute ether, and never had any difficulty in causing it all to pass into solution, except in the instances I mentioned in my previous paper, when a white substance was precipitated from the ethereal solutions of fat from human milk. This

substance when separated could not be redissolved in ether as I mentioned, and for the moment I thought it must be casein or sugar, but when I found that it melted and looked like any other hot grease the moment it was warmed, I concluded it must be some form of fat for whose reactions and appearance I could not account, nor can I now. The reason Dr. Leeds found milk-sugar with the fat when extracted by my method, must have been that he was careless in pipetting off the ethereal solution of fat, and drew off also some of the water containing milk-sugar, an accident which can only be avoided by observing the greatest care in that portion of the process.

There is little that need be said with regard to the objections brought forward to my method of separating the casein and sugar. Dr. Leeds says casein (albuminoids) is "partly soluble in boiling water." If this is true, my whole method must fall to the ground, but in my paper I stated my conviction, that if casein be once thoroughly hard dried, it entirely ceases to be soluble in water, and this is very easily tested by drying some casein and then trying to redissolve it.

It now becomes necessary to speak of the method of analysis advocated by Dr. Leeds, and of his detail of the work he has done. The method he follows is the Gerber-Ritthausen's, and he says of it, "that it is the only method known at the present time which is precise and rigidly accurate."

This method is described in full in a book entitled *Chemical and Physical Analysis of Milk, Condensed Milk, Infants' Food*, etc., by Dr. Nicholas Gerber, translated by Dr. Hermann Endemann, New York, 1882. The method, as described by Dr. Leeds, is as follows:

"*Totals Solids.*—Weigh off 5 grms. of milk in a tared covered platinum capsule. Coagulate with absolute alcohol (about 3 c.c. are used), and evaporate to dryness on water-bath. Transfer to drying-oven, and keep at 105° C. until constant weight is attained.

"*Ash.*—Ignite the residue first over a small flame, and finally at a dull-red heat. Cover the dish, cool in the desiccator and weigh.

"*Albuminoids.*—Dissolve 63.5 grms. pure sulphate of copper in a litre of water. Prepare also a potash solution containing 50 grms. caustic potash in 1 litre. Weigh out 10 grms. of milk in a covered beaker-glass, and dilute with 100 c.c. water. Add 2.5 to 3 c.c. of the copper solution. Then run in sufficient potash to neutralize exactly the excess of sulphate, which will require about 1.25 to 1.5 c.c. of the potash. The coagulated albuminoids settle immediately, leaving the liquid clear. In testing the reaction, the stirring-rod, which has been washed and withdrawn from the solution as soon as the potash has been stirred in, is dipped into the clear supernatant liquid. A drop of this liquid should turn neutral test-paper neither blue nor red. Care should be exercised not to allow the stirring-rod to bring up particles of the coagulum, since these interfere with the reaction. The clear liquid is then decanted through a filter-paper, previously dried at 110°, and weighed in a weighing flask. The precipitate is then stirred up with 100 c.c. water, allowed to settle, the supernatant liquid again decanted through the filter, and, finally, the precipitate is washed upon it. The beaker is thoroughly cleansed with a rubber washer; and all these filtrates, amounting to about 240 c.c., are finally made up to exactly 250 c.c. for the determination of milk-sugar.

"The filter-paper containing the precipitate is then opened out upon a large watch-glass, and, after drying to a certain point, is divided up into small particles by a platinum spatula, and this comminution is repeated from time to time, until finally the whole mass becomes a fine powder.

"*Fat.*—The filter-paper containing the precipitate is gathered up and placed loosely in a proper funnel. The beaker-glass used for the precipitation is washed out with ether to dissolve any traces of fat adhering to it, and these ethereal washings are poured through the funnel and allowed to run into a small weighed flask, with which the funnel is connected by a ground-glass joint. The funnel is then connected with a return cooler, the flask carefully heated by a water-bath, and the filter paper is made to swim in the ether condensed in the funnel for about an hour, when the extraction of fat will be complete. The ether is distilled off, the flask dried at a temperature of 105°, cooled in a desiccator, and weighed. Its increment in weight gives the amount of fat.

"*Albuminoids.*—The residue in the filter is dried at 110°, and weighed in the weighing flask until constant weight is attained. It is then ignited in a platinum crucible, and the weight of ash deducted. The loss of weight is the amount of albuminoids.

"*Milk-sugar.*—This is determined in the filtrate by Fehling's solution. The figures thus obtained are identical with those found by evaporation of the filtrate to dryness, igniting, and subtracting ash."

The first and most important objection to this process is that there are added to the milk to be analyzed fixed substances, which are believed to form new compounds

with some of its constituents. Thus, Dr. Leeds tells us, that an albuminate of copper is formed and precipitated. In his criticism of Haidlen's method he adds a foot-note, saying: "The addition of gypsum, marble, glass, sand, etc., is unnecessary, and a source of error." Why is not the addition of potash and sulphate of copper a much greater source of error? The first mentioned substances are comparatively inert, and little disposed to form new combinations, while the latter are among the most active in the strength of their chemical affinities. No method of analysis can be accepted as conclusively accurate which depends upon the introduction of a reagent which forms a new compound which is insusceptible of being again separated into its original parts, to show how much of each is present when the analysis is completed. How is it possible to know that the incineration of any given weight of albuminate of copper, formed from human milk, which has been dried at 100° or 105°, will inform us how much casein was originally needed to make up the compound, when it is remembered that casein itself is a substance about which we still know so little that it is not yet positively certain whether it is simple or compound?

Dr. Leeds's reagents for precipitating the albuminoids are almost exactly those used to make the Fehling's copper-test. Why, therefore, is it not likely that some of the sugar is precipitated or altered by them, as would surely happen if the fluid was heated, thereby preventing its reacting to the Fehling's solution, with which, as I understand it, Dr. Leeds made his test of the purity of his supposed casein residue. Dr. Leeds himself suggests another objection, "that, in the precipitation of the albuminoids by Ritthausen's solution, hydrated basic

sulphate of copper is precipitated." But, he says, this objection does not hold good, as he has "failed to detect in it the presence of more than traces of hydrated basic sulphate." For my own part, I made the experiment of mixing the reagents, as directed, in water simply, without the presence of milk. So soon as the copper and potash solutions were brought together, a precipitation of course took place. This precipitate I collected and dried at 100°, and then exposed to the heat of a gas flame. The difference between the weight of the precipitate dried in a water-bath at 100°, until it ceased to lose weight, and after it had been exposed to the heat of a flame, was (the same bulks of the reagents being used as directed for analysis) 0.038 gramme. This makes, if the same thing occurs when the method is used for analytic purposes, the casein too great in quantity by about 0.35 per cent., which is nearly the difference between Dr. Leeds's conclusion and my own as to the quantity of casein in human milk. Although the conditions are different, when the reagents alone are used, from those where they are used in analysis, and it cannot be argued that there must necessarily be a too high rating of the casein from this cause, yet, on the other hand, it can be by no means proved that such an error does not result; and, of the two possibilities, the latter seems much the more probable. This matter alone, even if there was no other objection to the process advocated by Dr. Leeds, is sufficient reason for declaring the method absolutely unreliable in the present state of knowledge of the subject.

I made experimental trials of the Gerber-Ritthausen's method, which Dr. Leeds declares to be the only accurate one known. The details of these trials it is hardly

necessary to describe here; the result was, that the Gerber-Ritthausen's method rated the fat and milk-sugar a trifle lower, and the casein higher, than my own method did when applied to the same milk. The ash and water came out the same by both processes. With regard to the ash, I found that it made no difference whether the incineration was done with an ordinary flame or with the blast, or whether in a closed or open vessel. If the burning was thoroughly done with the ordinary flame, the application afterwards of the blast did not drive off anything more; nor was there any difference in the weight of the residue, whether the vessel was open or covered. With regard to the fact that Dr. Leeds's process gave a slightly less amount of fat than did my own, although the difference was so slight as to be hardly worth mentioning, it struck me that, perhaps, the potash which is used may react to a slight degree upon some of the fat, and carry it down in some new form with the casein, which is acted upon by the copper. This is only a further reason for declining to accept conclusions deduced from a process which is open to so many sources of error, no one of which can be really proved to be inoperative.

The next matter I have to take up in connection with the work Dr. Leeds has done in milk analysis, is one which I approach with a good deal of hesitation, nor would I mention it at all in a spirit of faultfinding criticism, but only because it is necessary to have an exact appreciation of the correctness of his work, in order that a just estimate of its value may be attained. The table which he gives and which embodies the results of his analyses of eighty samples of human milk, has in it more errors than should exist when deductions are drawn intended to convince scientific men of the correctness of

an estimate of the composition of human milk which, to the present time certainly, has not received anything like universal, or even general acceptance. I have carefully gone over this table and find that in seven out of his sixty-four separate analyses, the figures in the column headed "total solids by addition of constituents" are incorrect. The errors are all small except in analysis No. 32, but in it the error is quite a large one. The errors occur in Nos. 24, 32, 46, 50, 54, 56–59, and 75–80, (marked, "anæmic six cases"). Dr. Leeds gives at the end of his table in parallel columns the maximum, minimum, and average of each of the different constituents. In the average column the figures given are all incorrect. It is not easy to say exactly how these averages were arrived at, for Dr. Leeds (page 243 loc. cit.) says that in estimating the "maximum difference" he omitted "Laboratory No. 1063" as being affected by some accidental error, and used sixty-two separate analyses, which would seem to leave out also the last two analyses, which are marked respectively, "robust six cases" and "anæmic six cases." My table shows in parallel columns Dr. Leeds's estimate of the average and corrected figures deduced in different ways.

		Leeds's average.	Average obtained by including all the analyses. Estimating Nos. 56–59 and 60–63 as 4 each, and the last two (Nos. 69–80) as 12. Dividing all the sums, except ash and total solids directly by evaporation, by 76, and those by 80	Analysis No 1063 omitted, otherwise same as previous estimate, but the sums divided by 75 and 79 respectively.	Analysis No. 1063 and the last two omitted, consequently the sums divided by 57 and 61.
I.	Specific gravity	1.0313	...	...	...
II.	Albuminoids	1.995	1.949	1.910	2.139
III.	Sugar	6.936	6.954	6.975	7.738
IV.	Fat	4.131	4.127	4.137	4.637
V.	Solids not fat	9.137	...	...	...
VI.	Ash	0.201	0.218	0.219	0.237
VII.	Total solids (by addition of constituents)	13.268	13.253	13.245	14.750
VIII.	Total solids (directly by evaporation)	13.267	13.237	13.235	14.637
IX.	Difference between VII and VIII	0 001	0.016	0.010	0.113
X.	Water	86.732	...	...	...

Dr. Leeds says: "thus, it will be seen, from the accompanying table, giving the results of sixty-two separate analyses of human milk (excluding Laboratory No. 1063, as being manifestly affected by some accidental error), the maximum difference is 0.21 per cent.

"The average error, as determined by ordinary arithmetical methods, is 0.001 per cent."

A glance at the table shows how far from the actual facts this statement is; for, if the first sixty-two analyses only are considered, and No. 1063 omitted, the error, instead of being 0.001 per cent., is 0.113 per cent., or just one hundred and thirteen times greater than is stated. The errors are all, it is but just to state, small, still the existence of so very many of them must necessarily cast much doubt upon the value of the whole work, especially when it is considered how very easily many of them could have been avoided, as, for instance,

in the matter of the making up of the column "total solids, by addition of constituents," when the sum in addition is so very simple a one.

I may sum up by saying that Dr. Leeds's conclusions must not be accepted, for the following reasons:

First, because he has failed to disprove any of the propositions I put forth in my previous paper, having brought no proof to bear substantiating his assertion, that the Gerber-Ritthausen's is "the only method known at the present time which is precise and rigidly accurate."

Second, that the reasons given for estimating the casein amount low (about 1 per cent.), still hold, in the absence of disproof.

Third, that the process advocated by Dr. Leeds is necessarily unreliable, because there are added extraneous fixed substances which form new compounds with the milk, rendering it impossible to make accurate determinations of the various constituents.

Fourth, because of the numerous and considerable inaccuracies which I have pointed out in the paper.

[After the reading of the preceding paper:—]

Dr. ALBERT R. LEEDS said: I shall refer briefly to some of the objections made by Dr. Meigs. The plan taken by Dr. Meigs of criticising the minutiæ of the work is an entirely just one, inasmuch as we propose to arrive at results of as great scientific accuracy as possible.

In regard to the first point that he speaks of. It is a little point, to be sure, but he is certainly not correct in stating that it makes no difference whether we draw off milk for analysis with a pipette or pour directly into the dish. Milk is not a homogeneous liquid in the sense that it does not contain minute particles which have the power of attaching themselves to the walls of the vessels, and the constitution of the milk after it has left the pipette is somewhat altered by the leaving behind on the walls of the pipette of some of these minute particles.

2d. In regard to the propriety of measuring a portion as compared with weighing a portion. There can be no doubt in the mind of any chemist as to which is the more correct. The only method which can lay claim to accuracy is that by direct weighing. He speaks as though it were a matter of difficulty to obtain an accurate weight. Such is not the case. When supplied with a beaker, which has its own tare or weight marked on the glass, a little experience enables one to estimate how much is to be poured in to make a definite weight of milk. The beaker is kept covered during weighing to prevent evaporation. When a measured quantity is employed, we cannot arrive at definite results until the specific gravity is ascertained. This method is so unsatisfactory that no chemist thinks of making a determination of milk analysis upon a measured quantity, but always upon a weighed sample. If, in a legal case relating to the adulteration of milk, it were discovered that the chemist had used measures instead of weight, I think that his testimony would be thrown out at once.

3d. As to the amount of time consumed in evaporation. A great many experiments have been made upon the relative thoroughness of drying by means of alcohol and drying without alcohol. The results are identical, provided one does evaporate to a constant weight. As a curious matter bearing on this point, I may say that my results upon the determination of total solids were checked off by other chemists

on the State Board of Health, who used other methods of drying. This seems to be a small matter, but inasmuch as the State law requires that in the State of New Jersey cow's milk shall contain above 12 per cent. of total solids, the method of drying becomes of importance. It was found that the results were practically identical, the only difference being that by the addition of alcohol a great deal of economy of time was attained.

4th. In regard to the use of ether as a solvent for the fat in the manner proposed by Dr. Meigs. I fail to see that there can be any justification whatever for its use in this way. Whenever it is necessary to extract fat from an organic body, the universal rule is, that even in a food substance like flour or meal, which contain very little moisture, previous drying of the organic substance should be resorted to before extraction of the fat with ether, because chemists regard the presence of so minute an amount of water as is found in food substances, as sufficient to hydrate the ether sufficiently to carry into solution other matters. This is as thoroughly verified as any point in chemistry. I look with perfect astonishment on a method of fat extraction which proposes to add 10 c.c. of ether to milk containing already as much as 37 per cent. of water to begin with, and then add two or three volumes of water in addition. You get very little ether and an enormous amount of water, and as a result other materials are necessarily extracted. It is not for me to go into what other analyses have been performed. I only speak of the analyses which I performed myself, and I found that the water had extracted other substances, and I should have thought it very remarkable if it had not done so.

5th. As regards the addition of foreign substances. In the early days of chemistry, at the very beginning of analytical work, it was considered that the bodies themselves should be actually separated by means of crystallization, different solubilities, and so on. These methods are now largely given up as lacking in accuracy. On the contrary, the great point in an analytical determination now is to obtain some compound of the body which we desire to isolate, this compound being one of very definite composition, and capable of complete separation from the other substances with which it is associated. The objection has been made to the addition of substances like copper, and to the addition of substances like sand, glass, gypsum, and so on. The objection to the addition of these latter substances rests on an entirely different ground from that which applies to the addition of copper.

When you add any of these latter substances no chemical union takes place, and the residue which is left behind after evaporation is highly hygroscopic. As direct evaporation gives all that is desired, they should not be used.

The addition of copper is resorted to for an entirely different object. It is added because it forms a most insoluble compound with the albuminoids and a compound which is capable of being manipulated, filtered, and washed with thoroughness. You may form an albuminate of zinc, of iron, or of other substances, but none of them, as has been shown by Ritthausen, who has made a more extensive study of the albuminoids than any other observer, possesses the facility for manipulating and for thorough washing as the albuminate of copper.

The action of potash on the casein could only happen in crude manipulations. In the manipulation of these albuminoids one obtains in the first place, in an acid solution, a precipitate of albuminate of copper; he then adds potash, not to such a point as to act on the albuminate, but to such a point as will just take up the sulphuric acid removed from the copper. It is added to the point of exact neutrality and in the presence of a strong mineral acid like sulphuric. I think that there is not the slightest danger of the potash dissolving the casein. Unquestionably it would do so if added to excess.

Then as to the albuminate of copper itself, the experiment detailed was of an entirely different description. On the addition of caustic soda to the copper solution, a precipitate of hydrate of copper would be obtained, which by boiling alone would give up its water and be converted into the oxide of copper. The experiment is not at all analogous to the changes occurring in the manipulation of albuminate of copper. When the albuminate of copper is obtained, it is washed thoroughly, dried, and then ignited, thus finding the exact weight of the mineral portion. This subtracted from the previous weight gives the weight of the organic constituents. I do not see that anything is lacking to make this mode of determining the albuminoids an accurate method. The presence of the basic sulphate of copper in the precipitated albuminate I have been unable to verify with certainty. The reduction of the copper in Fehling's test is a matter of an entirely different description. That is dependent upon the formation of a suboxide of copper. There is no trace of reduced copper when albuminate of copper is precipitated as proposed in the analysis of milk, and hence there can be no reduction of any substances of the nature of milk-sugar.

In regard to the small errors in addition, this is a matter of entire news to me. I am much surprised to learn that such is the case, but as to the conclusions being shaken, that I cannot suppose there is the slightest ground for believing on the strength of the evidence afforded to-night. The accumulation of testimony has been wholly adverse to the conclusions obtained by Dr. Meigs. They are unsupported by any other chemists who have worked on this subject. So far as I know, he is the only one who has advocated his view as to the small percentage of casein which he supposes to be present in woman's milk.

Dr. Meigs: I merely wish to reply to one thing. The remarks of Dr. Leeds would seem to show that I had taken a measured instead of a weighed quantity. I always weigh the quantity. I use the measure simply to get at the neighborhood of the weight. I use the pipette just as Dr. Leeds uses the beaker-glass.

THE BASAL PATHOLOGY OF CHOREA.

By

H. C. WOOD, M.D.,
CLINICAL PROFESSOR OF NERVOUS DISEASES IN THE UNIVERSITY
OF PENNSYLVANIA.

[Communicated May 6, 1885.]

I HAVE come to some definite conclusions concerning the basal pathology of chorea, which I should be glad to communicate to the College, and give the grounds for my belief.

The first point which I wish to make is, that the term chorea is simply one which is analogous to the term paralysis, and that the choreic movement is no more the same thing, necessarily, in its basal pathology, than is palsy the same thing in its basal pathology. When we come to study the various forms of disease closely connected with the choreic movements, we find, in the first place, the so-called cerebral or post-hemiplegic chorea, in which, after cerebral palsy, there appear violent convulsions with choreic movements. The seat of this lesion has been assigned by Professor Charcot to the corona radiata, near the lenticular nucleus; and there have been a number of post-mortems made which verify this view of its location. There can be no doubt that, in many cases, the lesion is situated in this position; but, on the other hand, it is equally certain that there have been cases of so-called post-hemiplegic chorea

in which the lesion has been in the external capsule and in the cortex. We may, therefore, say that this post-hemiplegic chorea is associated with various lesions in the brain, so far as seat is concerned.

Then, again, there has been reported in the *Compt. Rend. Société Biologie*, of Paris, a case of typical post-hemiplegic chorea, having absolutely all the features of organic hemiplegia with chorea, in which the most careful examination made by thoroughly competent persons failed to show any lesion whatsoever of the brain. As there had been a previous history of hysteria, the patient dying of pneumonia, we are forced to the conclusion that we may have post-hemiplegic chorea, which is without lesion, and hysterical in its nature.

When we come to study the more general forms of chorea, leaving out of sight for the present the chorea of childhood, or St. Vitus's dance, we have a chorea which is plainly hysterical; we have the electric chorea of the French, which I believe to be another form of hysterical chorea. We learn that, therefore, we may have a chorea dependent upon lesion of the brain, which also may have various seats, or we may have a chorea independent of any lesion whatsoever in any portion of the nervous system.

When we come to study chorea of childhood, we find that the pathology is perfectly parallel to that of the other forms of chorea. We have a large number of reported cases in which the lesion was evidently capillary embolisms of various portions of the brain, especially of the corpora striata and optic thalami, but also in other portions. We have other cases in which no lesion was to be found. Then we have recent cases, especially reported by Dickinson and Ross, in which

serious lesion was found in the spinal cord. The clinical history of chorea in childhood also shows conclusively, to my mind, that it may exist without any organic lesion which can be detected. We have it developing in a moment from emotion, passing off in a few weeks, and affected immensely by a few doses of arsenic or other agencies which would be powerless to affect any severe organic lesion.

So far as the study of the disease in the human subject is concerned, I think that these remarks sum up all that we can reach to, namely, that we may have chorea with various lesions and without lesions.

I have been much interested this winter in studying chorea in the dog. I found that, by offering a small sum of money, I could obtain all the choreic dogs that were needed. I made a careful study of the subject. It has been affirmed that chorea in the dog is different from chorea in the child. The great reason for believing that the disease is distinct is, that in the dog the movements are chiefly rhythmical, whereas in the child the movements are not usually rhythmical; but I have seen dogs with absolutely arhythmical chorea, and with all the awkwardness of chorea of children; and, occasionally, we have more or less of the rhythmical type in children. When we come to look at the points of resemblance in the two diseases we find, in the first place, in each case it especially affects the young animal. In the second place, that in each case the disease is associated with a constitutional disorder—distemper in the dog, rheumatism in the child. In the third place, the symptoms are exactly analogous, except that there is more tendency to rhythm in the one than in the other. In the fourth place, the clinical experience of veterinarians and of physicians

has led to the same result, namely, that arsenic is the best remedy that is known for chorea in the dog, and that it is the best that is known for chorea in the child.

Unless, in the case of a contagious disease, where you can pass the disease from one animal to another, it is manifestly impossible to be perfectly positive that a certain disease in the animal is the same as a certain disease in the child; but, when you find the habit of the affections the same, the symptoms parallel, and the therapy identical, you have as good reasons as can be possibly assigned for believing that the diseases are closely similar. It is, however, not necessary for my present purpose to take it for granted that the diseases are the same.

When I came to study chorea in the dog, the first point to settle, was the seat of the lesion. I therefore cut the spinal cord so low as not to interfere with breathing. I found that, invariably, the choreic movement continued after section. Before the section the motions of the front and hind legs were synchronous; that is, a wave of motion starting in the front paw would pass down the hind foot; but after the section this synchronous movement was wanting. The hind legs were completely isolated from the upper portion of the nervous system, and yet continued to exhibit the choreic movements, proof that the movements originated in the spinal cord. I therefore had my search for the seat of lesion narrowed down to a small fragment of nerve-tissue. I found that not only did the movements originate in the spinal cord, but that they originated, in all probability, in the motor cells, because when I galvanized the bared sciatic nerve, although the animal exhibited no signs of pain, the movements in the hind leg were at once

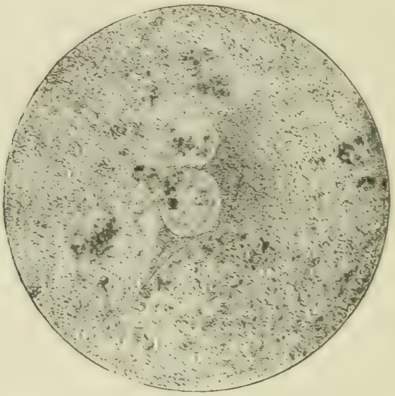
inhibited. The galvanism of the sciatic nerve could only affect the motor cells. Therefore I came to the conclusion, physiologically, that we had to do here with movement which originated in the motor cells of the spinal cord.

The next step was to examine the cord. Gowers and Sankey found in the cord a peculiar infiltration with leucocytes, and they came to the conclusion that leucocytal infiltration was the basal pathology of this disorder. Gowers had previously examined a cord without finding this condition. I examined a spinal cord and thought that I had found Gowers's lesion, but I was careful not to say so positively, because I had a suspicion that what was supposed to be a lesion was simply a peculiarity of the dog's cord. When I examined the cords of healthy dogs I found the same leucocytes. I do not say that what I saw was the same as the condition described by Gowers.

When I first examined the cord I perceived nothing wrong beyond what has been noted, but, bye-and-bye, as I began to study the specimen more and more closely, I found that the motor cells refused to take the carmine and hæmatoxylon staining as they should do. Then I remembered that I had killed the dog early in the disease. Then I took the cord of a dog which had died of the disease. Under these circumstances the lesion in the motor cells was very marked. They were crumpled up, the processes were gone, and the nuclei had disappeared. They were merely masses of matter, taking very little staining, just enough to show that they were protoplasmic. As I killed dog after dog in different stages, I found that the motor cells were to be noticed in all stages of degeneration. First, the perfect cell, then the

cell which stained badly, then one with nuclei disappearing, the margins becoming obscured, the processes dropping off, and opacity occurring; and, finally, the irregular protoplasmic balls. In a few cases I noticed peculiar degeneration—*i.e.*, the formation of vacuoles in these cells. (See figure.) A change, then, in ganglionic cells is what I believe to be the basal lesion of chorea.

It so happens that, some years ago, Putnam, of Boston, studied chorea in the cat. In the first case he found



no lesion, but in the second he found the lesion which I have described. The important point is, that he found that this lesion pervaded not only the spinal cord, but the whole nervous system. We all know that the choreic movement is only a part of the symptoms of the disease, and that the moral and intellectual part of the child also suffers. I believe that in children we have an altered condition of the ganglionic cells. The reason that no lesion is found early is, that the disease is at first functional. It is not necessary that I should call atten-

tion to the fact that there is no such thing as functional and organic disease. The line which we draw is a purely arbitrary one; for, the moment there is altered function, there is altered protoplasmic change, and, when there is altered protoplasmic change, there is altered nutrition, and then organic disease. Our microscopic eye is so blind and gross that it cannot see these fine changes until they become so distinct that we can detect them, and then we say that organic disease exists.

The history of chorea I conceive to be this: Owing to emotional disturbance, some stopping up of various vessels of the brain, or sometimes to the presence of organic disease, now this cause, and now that, there is an altered condition of the ganglionic cells throughout the nerve centres. If the cause is removed, and the altered condition of the nerve cells goes only so far, it remains what we call a functional disease. If it goes so far that the cells show alteration, we have an organic disease of the nervous system.

The capillary emboli, the clots, the tumors, and the various gross lesions which have been found in chorea, are not, in my opinion, the basal lesion, but the cause which produces these changes in the cells which are at the bottom of the choreic movements.

[A number of micro-photographs were exhibited.]

[After the reading of the preceding paper:—]

Dr. S. Weir Mitchell said: Did I understand Dr. Wood to say that he considers the lesion of the spinal cord as essential to the production of chorea, or does he think that there may be chorea dependent upon a descending lesion? I hope that Dr. Wood will take an opportunity of exhibiting the specimens, which will give us a better idea than the photographs.

Dr. Wood: In my detailed account, I shall give cases in which descending degeneration is present.

Dr. Osler: I might be allowed to say a few words on the subject, as owing to the kindness of Dr. Wood I had an opportunity of examining these specimens. I cannot say that the specimens carried as much conviction to my mind as have the words of Dr. Wood, to-night. At the same time, the changes which he has described are undoubtedly present. The only question is as to the interpretation of these appearances; whether they are really the result of the disease, or whether they are the result of methods of preparation, is extremely difficult to determine. In regard to presence of vacuoles, they have been seen and described before.

We all know that the appearances in the nerve cells of the cord are, above all other things, most deceptive. It strikes me that these appearances are not altogether conclusive to prove that Dr. Wood has arrived at the true essence of the pathology of the disease.

A PLEA FOR THE MEDICINAL USE OF PURE ALCOHOL AND ALCOHOLIC MIXTURES OF KNOWN COMPOSITION IN PREFERENCE TO ORDINARY FERMENTED LIQUIDS.

By

HENRY LEFFMANN, M.D.

[Read June 3, 1885.]

I PRESENT to the College this evening, with some misgivings I confess, a topic which can scarcely yet be considered a "live issue" in clinical medicine, but which is destined, I am certain, to become one. At the present time the profession does not take kindly to suggestions having in view material modifications of its policy in reference to alcoholic liquors. The majority of physicians regard those who preach or practise total abstinence, or throw doubt on the indispensability of alcohol as a therapeutic agent, as entitled to little respect or tolerance. In presenting the view that we should abandon in clinical medicine the use of the natural wines and liquors, and resort to mixtures confessedly fictitious, we must expect to encounter all the force of the conservative spirit. Many centuries of constant use have developed in the race a feeling that fermented drinks, particularly those that, like wines and malt liquors, have suffered no modification by distillation or admixture, are bounties of nature wisely given for our use. The traditions of the past associate the first preparation of liquor with

the gods, and in all ages poetry and prose have combined to increase the reverence for these natural products. Yet all this feeling is nothing but a superstition. Fermentation is now known to be a process occurring under the influence of micro-organisms, and it allies itself with ordinary putrefaction. The reverence which we have for "nature's laboratory" is born of ignorance, and there is no progress in chemistry more gratifying in its results, than that which deals with dispelling the illusions which have surrounded its application to medicine.

Whatever ulterior relations the plan advocated here may have to the questions of total abstinence are not presented for discussion; I merely offer it as a contribution to the methods of exactness and certainty in clinical work.

In the medicinal and dietetic use of fermented liquors, it is the effect of the ethyl alcohol which is sought to be obtained. It is true that those who prescribe liquors a great deal are in the habit of saying that the accessory ingredients, compound ethers, astringents, or bitter principles, etc., are also efficacious, but if we closely observe the customs of such prescribers, it will be found not only that the effect expected from the alcohol outweighs that to be obtained from any other ingredient, but also that in the majority of cases the accessory ingredients are either not known or recognized.

This fact is then taken as a starting point, that an agent universally recognized as one of powerful physiological activity should be used only in the most definite condition.

The forms of fermented liquors are numerous, and each form is subject to minor variations, depending on

on locality and season. The demand exceeds the supply, and hence the strong temptation to dilute and substitute. Within the past few months further notice has been given of the communications by American consuls abroad to the effect that the wines and brandies exported from France and Portugal are fictitious articles, in the majority of cases, and it needs but a little inquiry to show that a very large trade in liquors, more or less spurious, is carried on over the entire world.

Chemical analysis still has much to accomplish in the study of fermented liquors, but enough is known to enable us to imitate their essential features. The tabular statement of composition gives us a long list of mineral ingredients, but we are reasonably certain that, besides the ethyl alcohol, the only ingredients that need attention are the traces of fusel oil, compound ethers, astringent and bitter principles, and the effect even of their accessories is often more on the mind than on the body.

I suggest first, then, that in all cases in which the general physiological effect of ethyl alcohol is desired, it should be given by prescription, in the form of a rectified spirit of standard strength. My friend Dr. A. W. Miller, who is familiar with this topic, both from the point of view of the pharmacist and physician, has suggested that such a standard, pure spirit be made officinal under the title *spiritus maydis rectificatus.* Such a suggestion is in the interest of clinical accuracy and safety to the patient. If the medical profession have any concern in the protection of the health and morals of the community—and it would certainly appear that it has great concern—no better opportunity is offered for good work than in reforming the widespread errors in refer-

ence to the use of alcoholic liquors. Where is the physician who would say to a patient, take a little laudanum or chloral every day, and leave to the patient or the druggist the duty of determining the dose, or the duration of the treatment? Yet every day physicians give similar recommendations in regard to liquors. The use of rectified spirits in prescriptions is to be recommended on the same ground that we give potassium bromide and iodide in accurate dosage, instead of the sea-water which contains them, or morphia and quinine instead of opium and Peruvian bark. Incidental to the therapeutic accuracy and moral safety which are involved in such practice, is the not unimportant question of cheapness. Many liquors command prices far above the actual commercial value of the ingredients they contain. A pure French brandy, for instance, costs $12 per gallon. Its place can be taken by a spirit of much less cost.

Several objections may be made to the plan of using the plain spirit. I cannot stop to consider the one which arises from a belief in the superiority of a natural product, from a view that that which arises from a natural process will be necessarily superior to anything artificial; this, as I have said before, is a superstition; but there are some suggestions which are really important. It may be that the accessory ingredients have some therapeutic value, and it has been said to me that while pure alcohol may easily be used during acute disease and in hospital practice, that in long-continued treatment, and as a dietetic, patients cannot be made to take it. In these cases the method to be pursued is plain. Let the alcohol be mixed with suitable accessory ingredients. If a combination of bitter tonic, sedative.

and stimulant is wanted, it can be prescribed, and so on. There need be no difficulty in the matter, because modern art in the preparation of fictitious liquors has reached such perfection that excellent imitations of the natural liquors are made, and these have the advantage of definite and known composition and greater cheapness.

It is not uninteresting to note here the general nature of this work. I have the samples to illustrate it. In the preparation of fictitious liquors three methods may be employed. 1st. The genuine liquor may be diluted with a suitable strength of pure spirit. This will give us a liquor differing but little from the original. 2d. The liquor may be imitated by adding to pure spirit coloring and flavoring ingredients. In many cases this will give a liquor substantially identical with the original. 3d. The liquor may be made up weak, and then taste and appearance of alcoholic strength be given by means of pepper and bead oil. The latter method is reprehensible, but the two former methods are, I hold, not injurious, and should be recognized.

In order to make the present state of the art more clear to the members, I submit herewith samples of artificial liquors, cordials, and flavors, kindly furnished me by Dr. A. W. Miller, of this city.

[Samples of rectified spirit, whiskey, brandy, Holland gin, chartreuse, and curacoa, were exhibited, also a number of concentrated flavors and coloring matters.]

[After the reading of the preceding paper:—]

Dr. A. W. Miller said: I have listened with much pleasure to the reading of this paper, for the subject is one in which I have taken considerable interest for a number of years, and I have myself written several papers on it. I doubt whether Dr. Leffmann is entirely correct in speaking of these liquors as being made by art. It is simply following a well-known law of commerce and bringing those products from countries where they are abundant to those countries where they are scarce. In making whiskey we use alcohol produced by fermentation of corn, which is the cheapest article from which it can be made in this country. This is passed through percolators containing charcoal, sometimes animal and sometimes vegetable, which absorbs all the fusel oil and coloring matters. When this process is carefully performed, we have an absolutely pure spirit, which is made of such strength as to contain fifty per cent. of alcohol by volume. To flavor this we import from Germany, where rye whiskey is one of the cheapest forms, the oil of rye, which is there a waste product in the rectification of rye whiskey. When this is diluted to a proper strength, it can be used as a flavoring material.

Brandy is made in nearly the same way. The flavoring material is obtained by distilling the refuse of the grapes, from which the wine is made, with sulphuric acid. There is only one pound of this obtained from a ton of the so-called mark. When this is properly reduced, it may be used as a flavoring ingredient. These are not the only ingredients used in flavoring, but they are all harmless in the small proportions used. Another of these flavors is acetic ether. This is also present in the natural product. The peculiar bouquet of high-priced wines is probably due to the presence of acetates, and to the products of oxidation of fusel oil, producing valerianic acid and subsequently valerianates of ethyl and amyl. These are present in an infinitely small proportion. Artificial rye whiskey contains only one part of amylic alcohol in ten thousand parts. Brandy only one in fifty thousand. In addition to acetic ether, there is formic ether in brandy, and also butyric ether. All these things are used by confectioners in flavoring candies, and, as far as I know, no one has suffered from their use, although they are used in larger quantities. There is another point, namely, that liquor-dealers insist upon having a wholesome article, while confectioners are not so particular.

The cordials which have been shown are made from the rectified spirit with the addition of aromatics and syrups.

The curacoa is almost an exact representative of the simple elixir of the pharmacopœia. This is a very useful manner of administering a mild form of alcoholic beverage, and is to be preferred on account of having the sanction of the pharmacopœia, and on account of having a definite strength. This is another point in favor of the use of artificial liquors. The rectified spirit always contains fifty per cent. of alcohol. The natural liquors vary greatly, sometimes falling to forty per cent., and sometimes, as in rum, reaching seventy-five or eighty per cent.

I might say here that the unpleasant taste of ordinary diluted alcohol is probably due to the amylic alcohol, which is more soluble in strong than in dilute alcohol. Not being thoroughly combined, it causes a disagreeable taste and odor.

The economical value of these substitutes has been referred to. The rectified spirit can be bought for $1.25 per gallon, and its therapeutic value is equal to that of brandy at $10 per gallon.

I have proposed the name *spiritus maydis rectificatus*, because it designates the particular kind of grain from which this alcohol is derived, and prevents it from being confounded with the spiritus frumenti, which is now officinal.

Dr. John Graham: At the Franklin Reformatory Home, some three hundred cases of alcoholism are treated annually. For the last two years none of the ordinary alcoholic drinks have been used, but we have employed rectified spirits variously medicated. In the mild cases, alcohol is not used, but in the severe cases it is. The results have been equally as good as when the ordinary liquors have been used. In devising these substitutes for ordinary liquors, we must be careful that we do not injure instead of aid the temperance cause by the introduction of new drinks. In regard to dose, we consider one teaspoonful of rectified spirits to equal two teaspoonfuls of brandy.

Dr. J. L. Ludlow: I would like to ask to what the term oil of cognac is applied? Also whether Dr. Miller has noticed any difference between the so-called California brandy and so-called French brandy? I have tried the California brandy and it struck me as though red pepper had been added to it.

Dr. MILLER: There are different varieties of oil of cognac. The best is that which is obtained by acting with sulphuric acid on the residue of the grapes after pressing out the juice. As I have said, there is only about one pound obtained from a ton of residue. It is a complicated compound of the higher ethers. Some artificial oils of cognac are made by the action of nitric acid on oil of rue, others by the saponification of castor oil or cocoanut oil and the subsequent decomposition of the soap thus formed by sulphuric acid.

As far as my experience goes, California wines and brandies are perfectly pure. Their low price offers no incentive to adulteration. It is well known that brandies from different localities have different flavors. The California brandy also probably never reaches the age of the French brandy.

Dr. WILLIAM HUNT: I have frequently made the observation that in low forms of disease where alcohol is called for, the odor cannot be detected in the breath as long as the patient is not getting too much.

CASES OF POISONING BY CARBONIC OXIDE DUE TO A DEFECTIVE FURNACE PIPE.

BY

JOHN GRAHAM, M.D.

[Communicated by J. M. DA COSTA, M.D., June 3, 1885.]

ON March 9, 1885, I was called to see the following cases of poisoning from the inhalation of carbonic oxide, the poisonous gas being conveyed by the hot-air flue from one of our ordinary cellar heaters to the sleeping apartments of the patients. Their history will show how necessary it is to overhaul our heating apparatus, from chimney top to ash pit.

The heater was situated in the middle of a large cellar; had been in use for a number of years, and each autumn had undergone some repairs. It consisted of the ordinary iron heater, enclosed by brick, forming a hot-air chamber. Inside the air chamber was a terra-cotta pipe, running from the heater, and penetrating the wall of the chamber, thence to the chimney, to carry off the products of combustion. This pipe was cracked for eighteen inches of its length, the crack being open half an inch; and, unfortunately, the crack was in the air chamber.

This break is now known to have existed for several years; but up to the time of which I write had apparently produced no worse effects than annoying the family with a smell from the heater. Something more

was needed to change the chronic poisoning into acute. The chimney furnished the addition needed. Like too many of its kind, it had several angles, when it might just as easily have been perpendicular, from cellar to roof. It formed part of an outside wall, and the frost had loosened some of the bricks near the top, which, falling down and lodging at one of the bends, partially obstructed it. Complaints came from servants that the heater drew badly, but the cause was never carefully sought for.

Steadily the disintegrating effects of frost and air on brick and mortar continued, until at the time of the accident the chimney was completely blocked. Of course, it is highly probable that on the day in question a more than ordinary quantity had fallen, blocking it suddenly, else we would have had our patients complaining of sickness as well as smells, which they did not.

The patients, Mr. R., aged 78 years, Mrs. R., aged 74, went to bed on the evening of March 9, about 10 o'clock. Both were in perfect health and good spirits, and the only thing Mrs. R. remembers noticing was that the smell from the heater was a little stronger than usual. Fourteen hours after, the servant, alarmed at their not appearing to her calls, broke in the door and found them unconscious.

When coal is burned with a free supply of air, which, of course, necessitates an unobstructed chimney, the chief product is carbon dioxide. When burned with a deficient supply of air, as when the outlet is obstructed, the product is carbonic oxide. The first is poisonous when inhaled in sufficient quantity; three per cent. of the latter has been shown to produce death in animals in

thirty-seven minutes. Carbonic oxide is a little lighter than air, and when heated ascends rapidly.

In the case of my patients, the obstructed chimney diminished the supply of air to the fire, and the resulting product was carbonic oxide. Its tendency was to ascend. The chimney allowed no exit, so the crack in the terra-cotta pipe gave out the deadly product, and it ascended by the convenient hot-air flue to the open register in the bedroom, which, with closed windows, contained its unsuspecting victims.

Carbonic oxide is tasteless and odorless. We cannot recognize it by any of the special senses. The products of ammonia, sulphur, tar, are what make up the odors from burning coal.

When found, Mr. R. was breathing rapidly with loud bronchial râles, and his skin was dark and congested. Both had vomited sometime during the night, and their mouths were covered with frothy mucus. Their extremities were cold; the pupils dilated, and insensible to light.

Patients were allowed plenty of fresh air. Whiskey was given by the rectum, also hypodermically. Ammonia was given by the mouth whenever they were able to swallow, and warm applications were made to their bodies.

In the case of Mr. R., the labored breathing increased, the blood became more and more carbonized, and he died thirty-six hours after the discovery of the accident, without regaining consciousness. There was no failure in his heart, the pulse being full, but increased in frequency. He evidently died from pulmonary œdema. His chest was dull on percussion, the lungs filled with fine râles. For a few hours before death, his temperature rose rapidly, and reached as high as 106°. He had no paralysis.

Mrs. R., when found, was pale and breathing almost imperceptibly. The same means were used in her case to produce reaction, and slowly and gradually the heart's action increased; by degrees the

intellectual faculties were aroused, and the patient with much effort was able to inform us that from the time of going to bed until the return of consciousness all was a blank. She had suffered no pain; remembered nothing. Her pulse at this time was 60, small and compressible. Respirations 14; temperature below normal.

She was kept in the horizontal position. Liquid nourishment was given, with small doses of whiskey, and Basham's mixture ℥ss four times daily. The brain worked slowly, and she spoke only when roused up, and then dropped back into semi-consciousness. The tongue was coated, and had a slight tendency to dryness; bowels costive; appetite poor.

Her improvement progressed slowly, but at the end of seven days she was able to sit on a chair by the side of the bed. The pulse increased somewhat in volume, but the patient remained drowsy, falling into the same dull condition, unless when roused by noise, of which she complained greatly, saying it hurt her head. She also complained of intense pain in her back, running down the legs. At no time in the progress of the case did her urine contain either albumen, sugar, or tube-casts. Her blood was not examined.

Patient continued in about the same condition for three weeks following the accident. The Basham's mixture having been succeeded by pills of carbonate of iron, and the whiskey omitted. Pulse 60 to 70; temperature varying greatly from slight causes, from 98° to 103.5°, but mostly normal; anæmia very marked.

On the morning of the twenty-first day following the accident, the patient, while sitting on a chair, was taken very ill. A deadly pallor overspread her countenance, the jaw dropped, the limbs relaxed, and she became unconscious, convulsive twitchings passing through her body. We quickly placed her in the recumbent posture, and, after a hypodermic of whiskey, she slowly opened her eyes and asked, in an excited tone, "What is the matter?"

The supposed cause for the sudden change, was that our patient's digestion since commencing the iron had been gradually growing worse; the tongue drier and more coated; the appetite poorer.

I had realized that this digestive trouble was from the iron, but continued it up to this time on account of her profound anæmia. I now reluctantly stopped it, and put her on dilute muriatic acid, ten

drops every three hours, with pepsin after food, and enjoined absolute rest in the recumbent posture. The temperature on reaction reached 102° ; pulse 120. The pupils were still further dilated ; the reasoning power was all gone ; answered questions with difficulty, and in monosyllables. The tongue became very dry, and it was almost impossible for her to protrude it. Stools were watery, frequent, and involuntary.

At this time the patient was seen by Dr. J. M. Da Costa, who advised the continuation of the acid, careful nourishment, and keeping the bowels open.

The treatment resulted favorably. In forty-eight hours the tongue regained its moisture, the appetite improved, and the dulness of the mind diminished, though the patient still continued to have delusions and hallucinations, and the memory for certain words, and familiar words, was poor.

On Dr. Da Costa's second visit, on the twenty-sixth day of her illness (April 3d), the acid alone having been continued in the meantime, our patient was much better, tongue moist and clean, bowels natural, and, excepting the anæmic appearance and mental dulness, she was doing well.

We then put our patient on the same diet, milk, beef-tea, mutton broth, etc., without solids, and stopping the muriatic acid, gave tinct. ferri chlor. 10 drops, liq. potass. arsenitis 5 drops, four times daily.

This treatment was continued off and on until April 19th, the forty-second day of her sickness, when it was finally abandoned. Its use for a few days was invariably followed by dry tongue, loose bowels, and increased mental disturbance. This was also the case when we tried the arsenic without the iron. On resuming the muriatic acid, the tongue would regain its moisture, the bowels become regular, and the mind somewhat improved, but the anæmia, as shown by feeble pulse, pale skin, bloodless conjunctiva, continued.

On April 19th I noticed twitching in her left forearm, which was firmly flexed on the arm, and the hand on the forearm ; the mouth was slightly drawn towards the left side. I then stopped the iron and arsenic and returned to the muriatic acid treatment.

The rigidity of the left arm was followed by partial loss of power in the same, the patient raising it with difficulty, and not being able

to grasp with it as firmly as with the right. Her mouth became drawn still further towards the left; it was, therefore, a case of alternate paralysis. She swallowed with increasing difficulty, and had involuntary movements of urine and feces. Sensation in the fingers remained good.

Patient remained about the same, except that the bowel movements decreased in number, and the tongue regained its moisture, after again substituting muriatic acid for iron and arsenic, until Dr. Da Costa's third visit, on April 26th, the forty-ninth day of her illness.

He advised the continuation of the muriatic acid. She also had an application of dry cups to the back of her neck.

The paralysis, which we concluded was probably caused by an effusion near the base of the brain, slowly passed away, but the anæmia was almost as marked as ever.

On May 5th we began the use of ferri phos. cum ammon. cit. grs. ij, dissolved in 10 ♏ of distilled water, hypodermically, daily, but as it was followed by some hardening of the cellular tissues, and produced disturbance of pulse and temperature, we abandoned the use of the iron hypodermically, and gave it by the mouth in two grain doses, morning and evening, also continuing the muriatic acid.

From this time the improvement was progressive. The appetite increased, bowels became regular, the left hand and arm continued to regain their power, the deviation of the mouth to the left steadily diminished, and the patient became interested in her surroundings, and asked and answered questions with increasing intelligence. The pupils gradually diminished in size, and on May 20th, seventy-three days after the accident, our patient was sitting up, and four days after was able to walk into an adjoining room.

[After the reading of the preceding paper :—]

Dr. J. M. DaCosta said: In regard to this case, which has been so thoroughly worked out by Dr. Graham, there are several points which strike me as being of special interest.

In the first place, the serious parts of the case came on late. For a time the patient was apparently doing well, and then, in the midst of the affection she is paralyzed with a comparatively rare form of palsy, with paralysis of one side of the face and of the other side of the body. This was preceded by muscular twitchings; the case seemed to be hopeless; but she got over the palsy in ten or twelve days.

Another point was the persistence of anæmia, with all that that implies, including anæmia of the brain. The aphasia, also, was very marked. This was all the more striking, because she was a woman of decided mental force and culture and ready expression. The extreme aphasia which was present for a time was probably due to low nutrition of a portion of the brain from anæmia.

Another point, and one which has been well brought out by Dr. Graham, is in reference to the therapeutics. Until quite late in the case the symptoms were aggravated rather than improved by the use of iron. Improvement always followed the use of muriatic acid, and it is to this and the steady nutrition that her recovery is to be attributed. Late in the case, a form of iron which I have found to be well borne, the phosphate of iron with citrate of ammonia, was well tolerated and well digested; but this may have been due to the fact that the patient had so far convalesced that she could digest iron, whereas previously she could not do so.

Dr. Henry Leffmann: Poisoning from carbonic oxide is essentially different from that by most other gases in the fact that a compound is formed by the carbonic oxide and the hæmoglobin which is very difficult to break up. The compound formed by carbonic acid and hæmoglobin can be easily broken up by increasing the tension of the free oxygen present in the blood, but the carbonic oxide compound cannot be. The anæmia was due to the modified condition of the hæmoglobin, and not to the absence of iron, and, therefore, did not yield to the ordinary remedies. An examination of the blood by the microspectroscope and a counting of the blood-cells would doubtless have yielded important results.

Dr. J. L. Ludlow: It may be of interest to mention the cases of two students of the University, who came near being suffocated by coal-gas some years ago. They were attended by Dr. Jackson, who sent to Dr. Hare's laboratory and procured a bag of oxygen. The lives of both were saved.

Dr. Frank Woodbury: I would regard both of the cases reported as mixed asphyxia with only partial carbon monoxide poisoning. A case of pure carbon monoxide poisoning is one of sudden death, thus differing materially from carbon dioxide. A cubic inch of carbon oxide is said to be sufficient to destroy life in an adult human being. These cases are interesting from the manner in which they occurred. Cases of acute coal-gas (*i. e.*, gases resulting from the more or less perfect combustion of coal) poisoning are not very rare. Not long ago, I saw a family of five who were nearly asphyxiated by sleeping in a room with the windows closed and a range burning in an adjoining room, the cover of which had accidentally been left off. All these cases readily recovered under the use of diffusible stimulants and oxygen. I could cite other cases, if needed.

Cases of chronic coal-gas poisoning are more frequent than is commonly supposed.[1] A prominent physician of London—Sir Andrew Clarke, I think—said in a lecture, some years ago, that half of his patients got sick and got well again without his knowing what was the matter—meaning, I suppose, to illustrate the truth of the fact that there are many minor ailments to which no distinctive title can be given. I think it possible that a large number of these slight disturbances of health are due to coal-gas poisoning. They may be transitory, or extend over a considerable length of time. A gentleman, largely engaged in his office, complained of frequent headaches, drowsiness, and a tendency to fall asleep when reading, mental sluggishness, depression of spirits, and of loss of appetite. This continued for some time, and it was feared that there was some grave disorder of the brain; but the special senses were unimpaired, the muscular reactions were good, the dynamometer revealed no loss of power, and the patient when away from his office for a few days enjoyed excellent health, although never robust. It was subsequently discovered that there was an obstruction in the flue of the range in the room below.

[1] Article upon "The Dangers from Coal-gas in Our Houses," by W. Thornton Parker, M.D., in Philadelphia Medical Times, March 21, 1885, page 450.

Owing to the obstruction, the gas penetrated the bricks into the heater which supplied the office and sleeping-rooms, in the manner described by the lecturer of the evening. The only thing which saved the gentleman from more serious consequences was a hobby for ventilation and the habit which he had of sleeping with open windows. After the obstruction was removed, the gentleman soon recovered his usual health.

We have heard a great deal of late about sewer-gas, and the danger of stationary washstands in sleeping-rooms, and drains in the cellars. Would it not be well to pay a little more attention to the dangers of coal-gas in our houses, and inquire into the state of the flues from ranges and heaters as possible sources of some of the minor ailments from which we suffer ?

OBSERVATIONS

ON

SUNSTROKE AND HEAT EXHAUSTION.

BASED ON THE RECORD OF FIFTY CASES ADMITTED INTO THE PENNSYLVANIA HOSPITAL FROM THE MIDDLE OF JULY TO THE MIDDLE OF AUGUST, 1885.

By

ORVILLE HORWITZ, M.D.,
RESIDENT PHYSICIAN AT THE HOSPITAL.

[Communicated, October 7, 1885, by J. M. Da Costa, M.D.]

In presenting this paper to the medical profession, the writer deems it proper to state that it is done solely with a view to call attention to the therapy of the cases of sunstroke and heat exhaustion which were received into the Pennsylvania Hospital during the months of July and August, for he cannot help feeling that a line of treatment which will bring about favorable results when the temperature has risen to 112°, is worthy of being recorded.

He acknowledges his indebtedness to Dr. Joseph Kirkbride, who was physician-in-charge of the medical wards of the hospital during the summer months, for permission to publish the results of the cases.

The first patient was admitted on the 16th day of July; the average of the thermometer being 95° F. in the shade.

The second case of sunstroke was received July 17, when the thermometer stood at 99.5° F. in the shade. This man was brought in at 11 o'clock A. M. He had fallen whilst employed in loading his cart with dirt.

He was unconscious and very restless. His breathing was noisy and labored; respiration 27; pulse 165, strong and full. Temperature 109°.

He was at once immersed in a tub of ice-water and removed, when the temperature fell to 99° F. He was then put to bed and covered with a sheet saturated with ice-water. Ice was applied to his head. A hypodermic of tincture of digitalis (♏ xx), with an anal injection of antipyrin, was administered.

At 11.30 a tendency to convulsions was observed. The temperature was reduced to 102° F. A second injection of antipyrin of gr. xx was ordered, which had the effect of reducing the temperature to $99\frac{3}{5}$° F.

Two hours after admission the patient began to regain consciousness, but he appeared to be greatly alarmed, and feared that those about him would do him bodily harm. He very soon became violently delirious, and a state of acute mania was developed.

He was cupped at the nape and behind the ears, and about ten ounces of blood were drawn. He now became quiet, when a quarter of a grain of morphia was administered hypodermically.

After the administration of the second dose of antipyrin the temperature never exceeded 99° F. The patient was discharged cured on the ninth day.

The individual who was brought to the hospital on the 21st July was admitted at 2.30 P. M.; the thermometer at the time stood at 100° F. in the shade. His friends stated that whilst assisting in laying the street cable he suddenly complained of violent headache, which was followed by vomiting. He drank freely of ice-water and fell unconscious on the street.

When admitted he was completely unconscious; his breathing was labored; his pupils dilated; his skin was of a dark reddish hue; the capillaries filling very slowly when emptied by pressure.

His temperature was 112° F.; pulse 162, and irregular; respira-

tion 33. His sphincters were relaxed, accompanied by involuntary discharges of feces.

Treatment.—At 2.45 P. M. he was packed in ice and a bucket of ice-water sprinkled over him with more or less force. Five minims of aqua ammonia, with twenty minims of whiskey, followed by twenty minims of tincture of digitalis, were administered hypodermically. In fifteen minutes the temperature was reduced to $99\frac{2}{5}$°. F.

The ice-water was now removed; he was then covered with a sheet wrung out of ice-water, ice being at the same time applied to the head. His respirations were quick and shallow.

He was now given one-sixtieth of a grain of atropia, together with twenty-five minims of whiskey. By 3.10 P. M. the temperature had risen to $104\frac{2}{5}$° F. His pulse being weak, the hypodermic of aqua ammonia and whiskey was repeated, and forty grains of antipyrin were administered per anum. At 3.30 P. M. the temperature was $99\frac{2}{5}$° F., a hypodermic of ether was administered, and the patient was again covered with a wet sheet. At 7 o'clock he was slightly conscious, when he was given ten drops of tincture of digitalis, with a hypodermic of twenty minims of whiskey, repeated every fifteen minutes. At 8 o'clock the whiskey was omitted and a small quantity of pancreatized milk, with lime-water, ordered. At 9 o'clock calomel (gr. x), bicarb. soda (gr. x), and bromide of sodium (gr. xxx) were given; the last-named article to be repeated every third hour.

The patient was somewhat dazed for two or three days; he was discharged cured on the twelfth day after admission.

On the same day, July 21, a farmer, who had been working in the sun, was admitted. He had driven his waggon to market, and on reaching Front Street was seized with a violent headache, accompanied by sickness at the stomach, for which he took a large dose of whiskey, and fell to the floor unconscious as soon as the liquor was swallowed.

At 1.30 P. M. he was brought to the hospital. On admission his temperature was 109° F.; pulse 158; respiration 30; pupils contracted; involuntary discharges of feces.

He was at once placed in an ice bath and given twenty minims of

tincture of digitalis with one-sixtieth of a grain of atropia. His temperature fell to 99° F., when he was removed to bed and covered with an ice-water sheet; his temperature soon rose to $104\frac{2}{5}$° F. Antipyrin, sixty grains, per anum, was ordered, and in addition twenty minims of whiskey, hypodermically, every fifteen minutes were administered.

An hour and a half after admission the patient was seized with violent convulsions. A half of a grain of morphia, with one-sixtieth of a grain of atropia, was administered. He was then placed under the influence of ether. The convulsions continued for the space of an hour, when the administration of musk, in ten-grain doses, per anum, was resorted to. The convulsions entirely ceased after the administration of the third dose of musk.

The individual gradually became conscious. Two days after admission meningitis was developed. He remained in the hospital under treatment for six weeks, when he was discharged cured.

On the 24th July a woman, who was a cook, was received into the hospital suffering from heat exhaustion. She was a strong, healthy-looking German. When admitted her temperature was 110° F.; pulse 160; respiration 35.

For the first hour after her admission she was treated by Dr. Horwitz; after that she came under the care of Dr. Penrose. She was at once wrapped in a wet sheet, surrounded by pieces of ice, and ice-water was sprinkled over her. A hypodermic of aq. ammon. fort. (gtt. v), tinct. digitalis (gtt. xxv), and subsequently a hypodermic of atropia sulph., gr. $\frac{1}{60}$, were administered. Under this treatment her temperature fell to 99°; pulse 80; respiration 25.

The patient vomited and purged continually. Sinapisms were applied to the chest, abdomen, and thighs, which caused marked reaction. After this, hypodermics of tincture of digitalis (♏ x) and ether (♏ x), and a suppository of ten grains of carbonate of ammonia, were administered. Ice was kept to the head constantly. This line of treatment was continued for fifteen hours. The temperature did not again rise above 100°. F.

The patient slowly and gradually recovered, but did not begin to move about the hospital yard until September 19.

July 26, a laborer was admitted, who had fallen to the ground whilst engaged in paving the streets.

When received he was unconscious; his breathing was stertorous; pupils contracted; temperature $108\frac{4}{5}$° F.; pulse 159; respiration 30; bowels relaxed; he vomited immediately after admission.

An ice-water bath was at once prescribed; a hypodermic of five drops of aqua ammonia administered, followed by tincture of digitalis, ♏ xx. In fifteen minutes his temperature fell to 99° F.

He was removed from the tub and covered with a sheet wrung out of ice-water. Ice was applied to his head. His respiration being short and rapid, one-sixtieth of a grain of atropia was ordered. Ether, twenty minims, hypodermically, was administered, and dry cups were applied to the posterior base of the lungs. His temperature now rose to $103\frac{3}{5}$°, when antipyrin (gr. xxx) was administered per anum.

One hour after admission the patient was seized with violent convulsions, for which was prescribed one-half of a grain of sulphate of morphia hypodermically, and he was placed under the influence of ether.

The tendency to convulsions continuing, a suppository of thirty grains of musk was ordered; to be repeated every half hour.

Under the influence of the first dose of antipyrin the temperature fell to $99\frac{3}{5}$° F.; at the end of a half hour the temperature again rose to 104° F. Sixty grains of antipyrin, dissolved in eight ounces of ice-water, per rectum, were ordered, with the effect of reducing the temperature to 99° F.

About three hours after his admission he became conscious, when he was given calomel (gr. x) with bromide of sodium (gr. xxx); the latter to be repeated every three hours. The patient was discharged cured August 8.

The foregoing are presented as types of the fifty cases that were admitted into the Pennsylvania Hospital during the months of July and August, with a synopsis of the treatment pursued.

Of those received into the hospital, *twenty-four* were

cases of sunstroke, and *twenty-six* suffered from heat exhaustion.

Of the *twenty-four* cases of sunstroke, *nine died. Three died* within *ten minutes* after admission, and cannot fairly be said to have been subjected to treatment in the institution.

Four died within *six hours* after admission. *Two died forty-eight* hours after admission.

Of the nine that died, *four were hard drinkers; two were strictly temperate*, and *three drank in moderation.* Twenty-one out of the twenty-four had violent convulsions; one had acute mania, lasting one hour and a half.

The maximum temperature was 112° F.; this patient recovered.

The minimum temperature was $94\frac{2}{5}$°; this was a case of heat exhaustion.

Twenty out of the twenty-four cases of sunstroke occurred between July 16 and July 26.

The largest number received on any one day was on Wednesday, July 22, when nine cases were admitted. The thermometer on that day stood at $93\frac{5}{10}$° F. in the shade; on the two previous days it rose to 100° F. in the shade.

But two opportunities for post-mortem examinations presented themselves. In one case, which resulted in death ten minutes after admission, the temperature being 109° F., congestion of the lungs and kidneys was found to exist, with slight injection of the arachnoid and pia mater. In the remaining case, the individual dying two days after admission, there were presented the usual evidences of commencing meningitis.

On examination of the blood, the corpuscles were

found shrivelled in a few cases, but in the majority the microscope revealed no change.

Albumen was present in the urine in all but two cases, and this condition continued for two or three days after convalescence.

TREATMENT.—The *antipyrin* was used in all cases, in large doses, with the effect to keep down the temperature after it had been reduced by the application of ice, ice-bags, and ice-sheets to the surface; it was employed in the form of enemeta, but the writer suggests that it would probably be more potent if used hypodermically. It was not resorted to unless the temperature showed a decided tendency to rise.

Musk seemed to be decidedly advantageous in controlling the violence of the convulsions; it was administered in doses of ten grains, and by the time the ten grains were given the convulsions, as a rule, ceased. This remedy was administered in sixteen out of twenty-one cases of convulsions, and in all these it was of marked and immediate benefit; the violence of the attack was rapidly abated, and soon ceased to exist.

Aqua ammonia, in doses of five drops, repeated as occasion required, doubtless saved several cases, when the patient was about to die from heart failure, when the heart-sounds were indistinct, and when the pulse at the wrist could with difficulty be felt.

Ether hypodermically acted as a better stimulant than whiskey; administered by inhalation it controlled the convulsions, acted as a heart stimulant, and improved respiration in a marked degree.

Bloodletting.—One individual was bled from the arm to the extent of twelve ounces; he died two days after from meningitis. Four persons were cupped at the nape,

or behind the ears; about eight ounces were abstracted in each case. They all recovered.

The individuals in whom bloodletting was resorted to were all strong, full-blooded, heavy men, with injected conjunctiva, the veins of their necks standing out prominently; the pulse being full and bounding; convulsions setting in early.

Dry cups, employed in the sunstroke cases, were valueless; but, in those affected by heat exhaustion, the benefit was well marked and immediate, the patients *invariably* regaining consciousness after their application.

Tincture of digitalis, in twenty minim doses, administered when the patient was first seen, acted as an excellent heart stimulant. The pulse at once became fuller and slower, the heart beating more regularly.

Quinine, used after antipyrin had caused the temperature to drop, was of marked benefit.

When the patients became conscious, *calomel*, gr. x, and *bromide of sodium*, gr. xxx, were administered,—the latter repeated every third hour for the space of forty-eight hours, or longer, depending on the condition of the patient.

[After the reading of the preceding paper:—]

Dr. H. C. Wood said: The use of musk, as detailed in the paper just read, is, I believe, new. Antipyrin has, however, been used in one of the New York hospitals, and a paper written thereon by the resident physician.

There is one point which is worthy of consideration by hospital authorities. I have noticed myself, in experiments on animals, that time is of the utmost importance in the treatment of sunstroke, and our clinical experience accords with this. If the moment the animal became unconscious, I reduced the temperature by cold, the animal invariably recovered; if, however, it was left for ten or twenty minutes, reduction of the temperature caused benefit, and usually return of consciousness, but there were almost always marked signs of an impaired nervous system, and in a large proportion of cases death from paralysis. In the New York Hospital, antipyrin was given to the ambulance surgeon, and thus the remedy could be administered at once. I myself think that in very hot weather the hospital ambulance should be provided, not only with antipyrin, but also with ice, and no time would be lost, the remedies being applied as the patient was being brought to the hospital. The patient could be half undressed and rubbed with ice, and antipyrin could be used hypodermically.

Very few writers report the time which has elapsed before treatment after the sunstroke; and without such report statistics are of little value, because one of the most important elements of the case is omitted.

Dr. J. M. Da Costa said: It is but fair to Dr. Horwitz to state that this use of antipyrin is, so far as I know, original. Looking at these cases, it will be found that they were treated in July, while the paper alluded to, which has escaped my notice, appeared in August; it is evident, therefore, that he had thought of antipyrin himself.

In regard to musk, I have been utterly unable to find any reference to its use in the convulsions of sunstroke, and I am glad to hear so distinguished an authority as Dr. Wood state that it has never been used before for the purpose. The use of opium, or rather of morphia, hypodermically, for the arrest of the convulsions of sunstroke, also originated, so far as I know, in the Pennsylvania Hospital, and was published some years ago.

REMARKS

ON THE

TREATMENT OF ROSE-COLD AND HAY FEVER BY COCAINE.

By

J. M. DA COSTA, M.D.,

PROFESSOR OF MEDICINE IN THE JEFFERSON MEDICAL COLLEGE.

[Read October 7, 1885.]

In a communication which I made to the College last December,[1] I suggested that cocaine ought to be advantageous in hay fever. During the past summer I have had several opportunities of carrying this thought into effect, and, as I see by some very recent journals, others, too, have employed the drug with the same purpose; altogether, I think, it has been sufficiently tested for us to welcome it as a very decided addition to our means of counteracting this most troublesome affection.

The first case in which I became familiar with its use was one of great susceptibility of the nasal mucous membrane, which I saw last spring in a Southern gentleman. It was more like rose-cold, strictly speaking, than like hay fever, but due to the same irritability of the mucous membrane, and always marked and most annoying with early vegetation. A two per cent. solution, which

[1] Published in the Medical News of December 13, 1884.

I afterward increased for a time to a four per cent. solution, gave him such comfort and relief, that I had some difficulty in inducing him to discontinue the remedy. He said that its local employ not only soothed the intolerable irritation and stopped the sneezing fits, but exerted a quieting influence over his whole nervous system, similar to what he experienced from small doses of morphia, to the influence of which he was very sensitive.

One of the most striking instances of success I had with cocaine was in a young lady who had been for four years a great sufferer with rose-cold, which always came on about the middle of May, and lasted until June. Later it became a hay fever, and attacks of troublesome asthma complicated the disorder during the summer and the early autumn, especially in the latter part of July and August. She is a young lady with a sound digestion, and calm nervous system. It has been her habit to leave her country home in summer to go to Newport, whence generally, after a month's struggle with asthma, she has been obliged to move to some of the hay fever resorts in the White Mountains. Her eyes and nose suffer much in the earlier stages of the complaint; there is, however, no sore throat. Later a bronchial affection and asthma appear. The main complaint in the earlier stages is from the intolerable sneezing. This occurs especially in paroxysms in the morning, and is apt to stop after breakfast, although on cool days it often continues all day long. There is, also, especially under such circumstances, much running from the nose. All her pleasures have been interfered with, and her life in summer rendered very miserable by the complaint. She had tried many remedies, both local and general, without effect. Late in May I prescribed cocaine for her,

a four per cent. solution, telling her if she had any throat irritation to apply the remedy also to the throat. This she did not find necessary, the cocaine injected every morning into the nostril by means of a medicine dropper, about five drops in each nostril, gave her prompt relief. It arrested at once the sneezing fits, and she was comfortable, even free from coryza, all day. Once in a while, especially on cool days, a second application in the afternoon was resorted to, but this was rarely required. The numbness from the application lasted about fifteen minutes, and she perceived it more in the throat than in the nose. The most gratifying result from the use of the remedy was, that it prevented the asthmatic seizures.

She passed her summer at Newport without discomfort, only employing the cocaine after a time occasionally, and as she thought she needed it.

The cocaine employed in these and other cases was a four per cent. solution. A weaker solution, I am convinced, rarely does good. A stronger solution may be found necessary, and, before abandoning the remedy as ineffectual, I should always advise an eight per cent. solution to be tried.

In one case in which I applied a four per cent. solution, cocaine had previously been used, but, I have reason to think, in a much weaker form. The patient, his physician told me, had had hay fever most violently for fifteen years. He was known all over the West as "the hay fever man." He had tried everything; cocaine, too, had failed to relieve him. The attack came on always on the 17th of July, and lasted, with great severity, for months; some bronchial catarrh, but very little asthma, accompanying it. On the 26th of September

he tried a four per cent. solution in the evening, and slept that night comfortably for the first time for months. He has since used from five to eight drops, thrown up the nostrils with a medicine dropper every evening, and always with the best results. He does not like to resort to it in the daytime, because he finds that the fluid passes down his throat, benumbs it, and makes his speech difficult.

Other than the effect just mentioned, I have not seen any unpleasant result from its use. I must, however, except the case of a young married lady, who found so much relief to her hay fever from the local use of a four per cent. solution, that she employed it a number of times daily. The consequences were increased vascular tension and violent and distressing headache.

There is, undoubtedly, an insusceptibility—in some a varying susceptibility—to cocaine locally used. Thus, in an elderly lady with rose-cold, in whom no local remedies act speedily, a four per cent. solution produced very little impression. I meant to try an eight per cent. solution, but, as she left the city, I had no chance, and am thus forced to record this case as a failure.

The manner of employing the cocaine is not without importance. It may be used with a small atomizer as a spray. But the readiest means is to inject from five to eight drops up each nostril, the head being thrown backwards; in some persons once, in most, twice daily, will be found sufficient. It will be necessary to instruct patients not to irritate the membrane by rubbing it needlessly with the glass tube, or pushing this up too far. Thus a patient who had hay fever for thirteen years, and who was at the seashore on the 17th of August when the hay fever came on, and in whom tinc-

ture of ignatia amara seemed favorably to influence its course, tried cocaine in one nostril only. He inserted the tube far up, irritated the membrane, and water ran from that nostril, which became sorer and more inflamed than the other. More judicious attempts produced better results, but he could not be persuaded to give the remedy a fair trial, owing to his first experience with it.

Its mode of action in hay fever is partly by the local insensibility it produces, partly by the contractions of the capillaries it induces. The effects are thus chiefly local. It will not arrest the bronchial catarrh or the asthma, which attend some cases; yet it is astonishing how it seems to lessen the tendency to these complications when early applied, and before they have got much headway. Is its action, then, not partly a reflex action? That the remedy is radical, and, strictly speaking, curative, I have not found; but that it gives great comfort, converts bad into light cases, enables those to stay at their homes who otherwise are obliged to flee to hay fever resorts, relieves much suffering and distress, I know and have fairly tested. In no case of rose-cold or hay fever ought cocaine to be left untried.

[After the reading of the preceding paper:—]

Dr. HARRISON ALLEN remarked: I am glad to hear Dr. Da Costa state that the effect of cocaine is inconstant within a narrow range in different individuals. I have observed the same fact. In endeavoring to account for it, I have concluded that the difference lies in peculiarity of the erectile tissue. Those persons in whom the tissue is sparsely developed are, I think, less susceptible to impression by the remedy than are those in whom it is well developed. I recall one case in which a four (as well as an eight) per cent. solution was used persistently without benefit. If, then, one has under observation nasal chambers with mucous membrane exhibiting but little erectile property (changing very little under any of the conditions, such as galvanism, which ordinarily constrict the capillary network), the remedy will give but little relief. I have had three such cases under care during the past summer. The shrinking up of the erectile masses places the nose in what may be called a normal condition, the air passing through at a normal rate and the irritated surfaces not touching each other. One of the cases in which relief was not secured, was that of a lady suffering from the annoyance due to complete occlusion. After applying the cocaine for half an hour there seemed to be a little relief, but it lasted only a short time. Notwithstanding these failures, I have no doubt that further experience will show the truth of the author's statement, that we have in cocaine a remedy which will, in the majority of cases, give relief.

Dr. H. C. WOOD said: In this connection, the observations of Dr. Lyons, of Detroit, may be of interest. He has shown that there are probably two or three alkaloids in cocoa leaf, and that the commercial alkaloid, cocaine, not rarely is composed of more than the one alkaloid, ecgonine, and perhaps a third alkaloid is present. The unexpected results sometimes obtained from the therapeutic use of cocaine, may possibly be due to the presence of one of these other alkaloids.

The Chairman, Dr. RUSCHENBERGER, asked: Can any one tell us whether or not caffeine is capable of taking the place of cocaine?

Dr. H. C. WOOD: I have made some experiments with caffeine on the eyes of patients, and found it to be without effect.

THREE CASES

OF

REMOVAL OF THE OVARIES AND FALLOPIAN TUBES (TAIT'S OPERATION).

By

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[Read November 4, 1885.]

THE following cases are put upon record as a contribution to an important operation, the usefulness ot which is assured in certain cases, but the limitations of which have not yet been perfectly well defined.

CASE I. *Uterine Myoma; Excessive Hemorrhage and Anœmia; Tait's Operation; Recovery, and Cure.*—Mrs. L., of New Jersey, æt. 42; married at twenty-eight; two children—the last born ten years ago; each eleven pounds; normal labors; no miscarriages; absolutely well and strong till three years ago, when her periods became gradually more prolonged and profuse. Now she is unwell three weeks out of every four, and the flow is often so severe as to saturate a napkin every fifteen minutes, besides large clots of blood. She is thoroughly blanched, weak, and anæmic.

November 25, 1881. Sent to me for consultation by Dr. Hollingshead. A tumor is visible in the hypogastrium the moment she lies down, and the abdomen is exposed; sound enters $6\frac{1}{4}$ inches. The

tumor is an interstitial myoma in the posterior wall, as large as a large fist, moderately tender and painful; no vegetations on the endometrium; no erosion of os; cervix not involved; uterus movable. Advised Squibb's extract of ergot (♏ xxx–xl), hypodermatically, daily in the abdominal wall for a month; if not then better, advised Tait's operation, as all other means had been previously tried by her attending physician. She was unwell at the end of November, when the ergot was first used. It gave rise to great pain and considerable local inflammation, with nausea and vomiting, and had to be discontinued.

December. Again unwell; the intermenstrual period was freer from pain; but she was weaker and more blanched, and not able to come to the city; lost a large quantity of blood.

January 26, 1882. Came to the city; was so blanched that had she closed her eyes and folded her waxy hands she could easily have been mistaken for a corpse; weight one hundred pounds. Treatment: iron, quinine, milk-punch; food every two hours.

28*th*. Taken unwell; period lasted till February 2; used twenty-eight napkins, besides passing a number of large clots. Bad neuralgia of face; morphia ($\frac{1}{8}$ grain), hypodermatically, failed to relieve, but water similarly given lessened it. Eats but little on account of pain.

February 3. Dr. R. P. Harris saw her with me, and concurred in advising the operation. Temp. 98.5°; pulse 80, feeble; heart normal, but weak; no change in uterus.

9*th*. Operation 12 M.; antiseptic method with carbolic acid, including the spray; bladder emptied. Duration of operation forty-five minutes. Ether (f℥vijss) used; incision four inches long in median line from pubes half way to umbilicus; no vessels tied.

On opening the abdomen a moderate amount of serum escaped. On account of the high position of the uterus the ovaries were readily found. Each pedicle was transfixed with a double carbolized silk ligature close to the uterus, the upper including the Fallopian tube, and after ligature the tubes and ovaries were removed. The left ovary showed a recent corpus luteum; it had a few small cysts, and was cirrhosed in part. The right had one cyst two inches in diameter, and several smaller ones. The right tube was cystic just at the

cervix uteri; it contained a serous fluid. The veins were very large; no bleeding requiring a ligature occurred. The ligatures were all cut off short; four deep and two superficial sutures, the former, including the peritoneum, closed the wound. Dressed with carbolized gauze.

Immediately after the operation her pulse was 120, and feeble. Hot-water bottles were applied, and brandy was used, hypodermatically, several times with good effect. She vomited only once up to 3.30 P. M., when her pulse was 93; temp. 97.2°; very small quantity of food and stimulant every twenty minutes. 7 P. M., pulse 100; temp. 99.6°; has had some pain; feels stronger. 11 P. M., temp. 100.8°.

10*th*. Slept but little, but is comfortable; temp. 100°.

11*th*. Temp. 98.4°; considerable pain in the back at 9 P. M. last night, followed by a bloody vaginal discharge. In twenty-four hours has used sixteen napkins, moderately saturated. Water at 105°–110° ordered, which gave great relief.

12*th*. Has used twelve napkins; temp. 98.8°.

13*th*. Slept excellently; discharge has ceased; dressing changed (fourth day). It was barely soiled, with very slight oozing from the operation; no pus; wound free from blush; union by first intention throughout; meat allowed.

15*th*. Several enemata having had no effect, as she felt uncomfortable, the rectum was emptied, mechanically, of a large amount of impacted scybala.

19*th*. Redressed; wound healed; sutures removed.

22*d*. Sat up.

25*th*. The menstrual period was due on 24th. Has used two napkins to date; less than f℥ss blood on each.

March 17. Went home; weight 110 pounds.

24*th*. Menstruation due; had some backache; no blood; staid abed three days.

June 6. Came to see me; brown as a berry; weight 130 pounds; appetite good; strength nearly regained; each month had had slight malaise; no bleeding; uterine cavity three and three-quarters inches; myoma not perceptible, except by bimanual examination.

March, 1884. Rapidly regained full strength; weight has con-

tinued at 140 pounds; no bleeding; sexual appetite unimpaired; tumor entirely gone; uterus three inches.

CASE II. *Severe Nymphomania, leading to Incipient Insanity; Menorrhagia; Tait's Operation; Recovery; Cure.*—Mrs. B., of New Jersey, æt. 42, American; eight children—last born eight years ago; operated on by me, successfully, four years ago for lacerated perineum, and later, another operation for severe hemorrhoids. Wife of a poor, ill-paid clergyman, and hence her life was a constant struggle properly to feed and clothe her large family. I have known her from childhood. She was always a most exemplary Christian woman.

Her head began to trouble her not long after the first operation, and she attributed it to the ether, which, however, she bore perfectly well in both operations. She had strange feelings as if unconscious, and in a fright or dread of ether, especially at night. Exposed to the sun, in August, 1881, she had an attack of heat exhaustion, followed by a second attack a week later. After this her menstruation, always previously easy and regular, ceased for three months. During this time she was treated for malaria, and her head became worse, which she attributed to the quinine. She became very nervous and sleepless; lost all self-control; could not bear any noise of the children, the church-bell, or even her own voice. She became unable to do any work, and had extreme depression of spirits; attempts at suicide were repeatedly contemplated, and though almost determined to end her life, she was deterred by her religious fears. These emotions were readily confessed to me and to her husband. In December, 1882, her menstruation became very profuse, and was continuous for three months. Since then it is not continuous, but is still very profuse.

Meantime, in October, 1881, by spells her sexual appetite, till then a matter of little moment, became immoderate. Day and night it was an exquisite physical and mental torment, and even led her to repeated self-abuse when it could not be gratified. This nymphomania and her head symptoms were always worst at her menstrual period. Finally, she went voluntarily to an insane hospital, in March, 1882, being utterly unfitted for her household duties, and in constant dread of suicide; but soon returned home.

January 2, 1883. I saw her; head still as described, and she was almost desperate; uterus normal, except some erosion at os, and freely movable; clitoris and other generative organs normal. Her attacks of nymphomania were still frequent and severe, especially during menstruation. She was fast passing toward permanent insanity. She loathed herself for her abnormal sexual appetite; she had struggled against it, as well as against her suicidal intent, till she was ready to hail anything that gave the faintest hope of relief at any risk to life, for which she cared absolutely nothing. She had been under varied and excellent care, and every moral means and all promising drugs had been freely tried. I therefore proposed Tait's operation, to which she and her husband instantly assented.

4*th*. Operation; ether; antiseptic method (carbolic acid), with spray; bladder emptied; incision three inches in median line upwards from pubes; layer of fat (she was well nourished) one inch thick, belly-wall two inches. Left ovary found without difficulty; its pedicle pierced by needle with eye in the point carrying a double carbolized silk ligature; ovary and Fallopian tube tied separately and ligatures cut short. One ovarian vein was varicose and as large as the little finger; ovary and tube both removed. Two pedunculated growths of the size of peas were attached to the ovary, one directly and another from the middle of a long foot-stalk attached at the two ends to the ovary and to the tissue between the ovary and tube. The right ovary was found with some little difficulty; as it was pulled out of the wound a small cyst burst. It was treated precisely as the left, and tube and ovaries removed. Both tubes and ovaries were intensely congested (her last menstruation was five days past); several small cysts existed in each.

Her recovery was uninterrupted. She had a little bilious vomiting and retention of urine requiring the catheter, but no pain; and no medicine.

8*th*. A moderate vaginal hemorrhage began, which ceased four days later spontaneously.

9*th*, 11*th*, and 14*th*. The stitches were removed. Her highest temp. was 99.4°.

19*th*. Down stairs.

23*d.* Went home. Since then I have seen her repeatedly; the last time in the spring of 1885. Her mental symptoms and head troubles have gradually become better. For the first six months or more she was often despondent, but she gradually recovered her cheerfulness to a large extent, resumed her household occupations, and is perfectly well. The nymphomania ceased from the time of the operation, save two very slight and short attacks. Coitus is rare, but is entirely normal, and is not followed by any tendency to her former deplorable condition.

CASE III. *Uterine Myoma; Severe and Long-continued Hemorrhage; Operation; Death.*—Miss W., æt. 40, first menstruated at fourteen, always profusely. For the last seven to eight years much worse, the flow continuing ten to fourteen days. In May, 1884, she began to suffer from continuous hemorrhage, which has persisted till the present date, January 2, 1885. Occasional severe hemorrhages also occurred. She is very pale and anæmic, with waxy lips, and has lost much flesh and strength, especially of late. To-day I examined her under ether: uterus three inches in length, and movable; a myoma as large as the fist was discovered in the anterior wall and fundus. Hypodermatic injections of Squibb's ergot, in f℥j doses, every second day, were used, to which, later, was added f℥j of the fluid extract of ergot daily, with tonics and good diet.

January 29. Has passed the menstrual period without noticeable hemorrhage, and to-day, for the first time since last May (excepting two days), has dispensed with a napkin. From this date till April her menstruation ceased. In April and May she had a normal discharge. But in June the hemorrhage returned, and continued so profusely as to threaten life.

July 4, 1885. The hemorrhage having been checked for three days by the above means, I operated. The tumor, which had clearly increased in size, was immediately seen on uncovering the belly. Ether; antiseptic precautions, including the spray (carbolic acid); bladder emptied. The enlarged uterus was so much in the way that the ovaries could not be seized through the small incision first made in the linea alba, the ovaries not having been carried up with it, and it had to be prolonged one inch above the umbilicus. The whole hand had to be introduced, the uterus lifted and pushed forcibly

aside, and the ovaries were even then reached with the greatest difficulty, and after several attempts. The ovary and tube on each side were removed, the pedicle being tied with stout carbolized silk, which was cut off short.

The left tube was attached to the ovary at the fimbriated extremity; two cysts, one filled with blood and one with serous fluid, existed in this ovary, the stroma of which was largely cirrhosed. One large (size of English walnut) and one smaller serous cyst were found in the right ovary, and its stroma was atrophied and cirrhosed. All four cysts were ruptured during removal. About eight ounces of serum were found in the peritoneum.

5th. The wound was united with silver wire sutures after careful cleansing of the peritoneal cavity (there was no bleeding), and then dressed with carbolized gauze. Symptoms of peritonitis began to develop, and in spite of all remedies progressed to a fatal issue on July 7. The temperature was 102°–103° till shortly before death, when it rose to 106°.

Autopsy, July 8. Recent lymph was found over a considerable portion of the belly contents, with an ounce of pus in Douglas's cul-de-sac. No hemorrhage had occurred.

THE ANATOMICAL BEARINGS OF THE SEROUS COVERING OF THE VISCERA.

By

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[Read November 4, 1885.]

In my remarks I shall deal only with the anatomical difficulties that perplex the student, and shall be content if I can present him with so clear a description of them, that he will remember them, because he understands them. I shall make few allusions to the works of embryologists, simply stating at the outset, that all that is known about the subject is due to their tireless researches.

At a very early stage of development, the intestinal canal consists simply of a straight tube, extending the whole length of the body of the embryo, and lying in front of that which subsequently becomes the vertebral column. It is closed at both ends, but communicates with the umbilical vesicle V, Fig. 10, which is external to the body. It is covered throughout its entire length by a continuous serous membrane, since at this stage of existence there is no horizontal partition, the diaphragm, to divide the body into two sections. At this period, the intestine antedates all the viscera, save the heart, and a cross section of any part of the body of the embryo,

whether through the thoracic or abdominal portion, will represent a tube (the body walls) enclosing a tube (the alimentary canal).

But this simple anatomical expression does not long continue. Soon at certain points of the *intestine* a budding process is manifest, and the *lungs*, *liver*, and *pancreas*, make their appearance, whose development with

Fig. 1.

In this sketch of the anatomy of the embryo, I have represented both tubes as complete, and the serous membrane as continuous. It forms a parietal layer to every part of the body of the embryo, and a covering to the viscera, which push forward into it. The *cavity*, which at first extends the whole length of the embryo, is called the pleuro-peritoneal, and the *serous membrane* the pleuro-peritoneal.

that of the diaphragm converts the simple expression in Fig. 1, into a labyrinth, every path of which the student feels, ends in fog, or leads to impenetrable darkness.

At a very early date the diaphragm makes its appearance. Nothing positive of its development has as yet been established. Being a muscular organ, it is likely that it springs from the body plates, and grows from the periphery toward the centre. Its appearance, it would seem, must be prior to, or coincident with that, of the liver, which can be clearly defined by the middle of the third day. Although all the various processes of development are moving on together, it will be simpler to speak of some of them as complete in advance of others. Thus, by the formation of the diaphragm, we may look upon the pleuro-peritoneal cavity, already described,

as separated into two distinct apartments, an upper, thoracic, or pleural; a lower, peritoneal, or abdominal.

It has already been stated, that in the early hours of development there are no lungs, and when these make their appearance, it is as buds or offshoots from the œsophagus, Fig. 10, L. A simple diagram, suggested by Balfour and Foster, will illustrate the steps of this process. At the site of the œsophagus where the lungs are to be developed, a cross section would show an elongation, Fig. 2 (A), later a double constriction (B), and

Fig. 2.

still later the lungs as independent structures (C), connected at first by separate tubes with the œsophagus; these soon fuse, and a single permanent trachea is formed.

In the demonstration of the pleural membrane, only the lungs and heart concern us, since these alone receive a covering from it, and by it are enclosed in separate apartments. In the description of this process, it will be simpler to regard the thoracic boundaries as complete in advance of their contents, and to represent the heart and lungs by three dots resting behind the serous membrane, Fig. 3. Thus we have the simple expression of

a single cavity, lined by a single and continuous membrane. Little by little these structures grow forward into the pleuritic cavity, and, as they do so, push the lining membrane before them. Though destined to

Fig. 3.

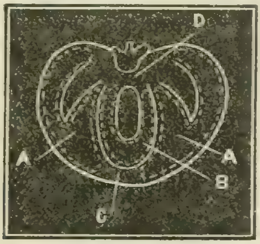
occupy nearly the whole of the thoracic space, they do not advance with equal rapidity. The heart exhibits functional activity in advance of every other viscus, and its rapid development and great importance give it at first much greater space than that required for the lungs, whose function in the higher orders is never called into action prior to birth. In Fig. 4 I have rep-

Fig. 4.

resented the lungs, and the heart with its pericardium, pushing forward and filling up the cavity of the chest, and just as I have represented the chest-walls as developed in advance of their contents, so I have represented, diagrammatically, the pericardium, in advance of the heart. Note that the viscera have not as yet separate compartments; that the serous lining in Fig. 3, can still

be traced as a single continuous structure in Fig. 4. A little later the pericardium meets and unites over the heart, while at the same time it unites with the sternum in front, and the diaphragm below, thus effectually isolating the heart and lungs, and creating three distinct serous cavities, A, A, B, Fig. 5. Were we now to

FIG. 5.



regard these processes—*i. e.*, the development of the chest-walls, the diaphragm, the heart, lungs, and pericardium—as all advancing together, it would be easy to conceive how the pericardium by its relations to surrounding parts—*i. e.*, the diaphragm and sternum—becomes the principal factor in constructing the partitions of the chest cavity.

At the outset the œsophagus was, in common with the entire intestine (Fig. 1), covered with a serous membrane. But for this circumstance and the one already mentioned, that the lungs sprout from the œsophagus, neither the lungs nor heart would have a serous covering. How, then, does the œsophagus lose its covering? Simply enough—*i. e.*, by the advancement of the heart and lungs. These structures *push the thin, delicately attached serous membrane before them, stripping it off from the œsophagus, trachea, and large vessels*, and appropriating it to themselves. Hence a space just in front

of the vertebra is formed that has no serous envelope, a space between the roots of the lungs and back of the heart (Fig. 5, D). This space is called the *posterior mediastinum*, and the only remark that I wish to make in regard to it is to *contrast its formation* with that of the *anterior mediastinum* (Fig. 5, C), which latter is formed by the *fusing* of the serous covering of the pericardium with that of the sternum and diaphragm. If this view is correct, then in cases of so-called congenital absence of the pericardium there can be no anterior mediastinal space, and no separate cavities for the heart and lungs, while the presence of these chambers and the isolation of their contents, must prove the statement of the so-called congenital absence of the pericardium to have been an error.

I will now pass to the abdominal portion, and take up the stomach and duodenum. It is from the *duodenum* that the *liver* and *pancreas* are formed, and it is by changes that take place, as development advances in these structures, that the foramen of Winslow, the posterior or lesser cavity of the peritoneum, and the four

Fig. 6.

layers of the great omentum are formed. Again, let me refer to the embryological fact, that in early life the intestine lies as a straight tube along the vertebral column, covered by the peritoneum. In order to represent the bowel as straight, I am forced to represent the liver and pancreas (see dots, Fig. 6) as widely removed

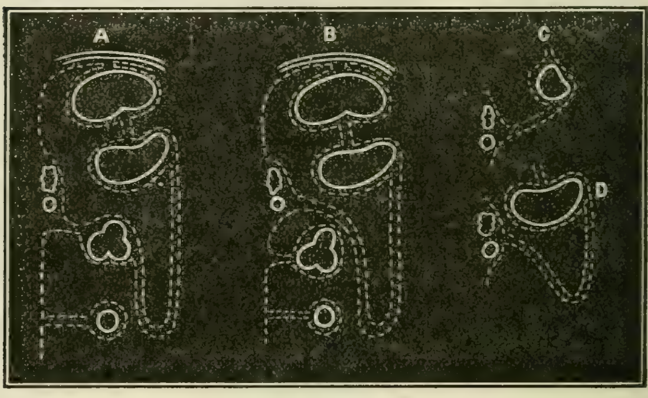
from the diaphragm. The liver, however, lies immediately beneath the diaphragm to the right of the median line, and, as it increases rapidly in size and weight, draws the lighter and less stable stomach and duodenum with it. Thus the liver, stomach, and duodenum, instead of continuing to look directly forward, with a layer of peritoneum on either side of them, gradually, so to speak, turn upon their right side, so that the liver lies in the right hypochondrium, and that which was at first the left side of the stomach becomes its anterior surface

Fig. 7.

(Fig. 7). For a time we may suppose all these structures to remain free and unattached, and that the hand can be passed freely beneath the duodenum, pancreas, stomach, and liver; but soon the transverse duodenum fuses with the serous membrane upon which it lies, and now a small cavity is formed behind these viscera, which communicates freely with the general peritoneal cavity along the free (right) border of the liver and descending duodenum. The opening is large at first, extending as it does from the level of the pancreas (Fig. 7) to the diaphragm, but as the liver and duodenum increase in size, and approach each other, and fuse more with surrounding parts, the opening is narrowed to a foramen, which bears the name of Winslow. The space behind the stomach and liver now is limited above by the dia-

phragm, below by the transverse duodenum, upon the left by the original mesenteric attachment, and at its free border closed, except at the foramen of Winslow. Although these viscera have changed their position, and lie, as it were, upon their sides, yet they still, in the main, retain the same relation to their serous covering —*i. e.*, they lie between its two layers (Fig. 7), one layer being in front and another behind. The two that connect the liver with the stomach are called the gastro-hepatic omentum; the two that connect the stomach and pancreas, the gastro-pancreatic omentum; the two that connect the stomach and spleen, the gastro-splenic omentum. Let us now suppose the stomach, finding

Fig. 8.

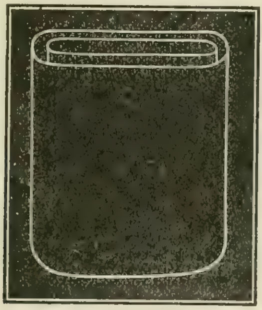


A. Incorrect; the great omentum encloses the transverse colon, and forms its mesocolon. B. Correct; the great omentum, sends its two posterior layers over the transverse colon to the pancreas and duodenum. C, D. Successive steps in the formation of the great omentum.

itself hemmed in within too narrow boundaries, to begin a struggle for more freedom. It cannot free itself from the diaphragm and liver, nor from its original mesenteric attachment; but finding the two layers that pass

from its greater curvature, to the spleen and pancreas, more yielding, tugs away at these until both layers stretch and hang down into the abdominal cavity (Fig. 8, C, D, B). This constitutes the greater omentum, which is nothing more than a stretching or bulging forward of the two layers (C) of the gastro-pancreatic omentum. As they increase in depth they may not inaptly be compared to two sacs (Fig. 9), one within

FIG. 9.



the other, the anterior layers of which, if followed upward, would enclose the stomach and proceed up upon the liver, and whose posterior layers will embrace the pancreas and duodenum. The space within the inner sac communicates, of course, with the space behind the stomach and liver, and may be regarded as merely an annex. Should these sacs fuse, it is plain the single membrane thus formed ought to be considered as consisting of four layers, just as the great omentum is described in the text-books.

I have been particular to describe the formation of the great omentum without mentioning the transverse colon, and this is the way it should be understood.[1]

[1] First accurately described by J. Muller, 1830: Cruveilhier and Sée, Splanchnologie, p. 537.

Upon this head most of the text-books are in error—not only in their diagrams but in the text. I have examined about a dozen text-books, and of these, Quain, Holden (1868), Cruveilhier, and Sée, are the only ones correct. The prevailing error has been occasioned by observing that the omentum is, in the adult, usually attached to the transverse colon, but while attached it does not surround it as in Fig. 8 (A), or form its mesocolon, but passes in front and above it (B) to the pancreas and duodenum, from which it in fact sprung (C, D). When correctly represented, as in B, the colon, as Professor Chapman states, gets upon its anterior and upper surface five layers of peritoneum—*i. e.*, its own mesocolic layer and four from the great omentum. In the text-books the same portion of the colon (A) gets only three layers. In other words, the upper surface of the transverse colon has five serous layers—its lower surface but one.

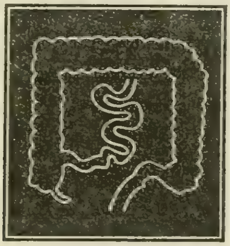
With the foramen of Winslow and the great omentum the chief difficulties of the peritoneum disappear, though some points about the colon are not without

Fig. 10.

interest. Of the early embryonic condition of the intestine, that depicted in Fig. 10 is very frequently given. I employ it to call attention to the growth of the bowel

and its extending forward, thus elongating its mesenteric attachment; soon a twist takes place of the *lower part* of the intestine *upon the upper*, by which, that which is to constitute the cæcal end of the colon comes to lie in the right iliac fossa, and that which is to constitute the sigmoid flexure to lie in the left iliac fossa, and the entire colon to form three sides of a square where it becomes attached to the periphery of the abdominal cavity (Fig. 11). This twist of the colon to the right, like that

FIG. 11.



of the stomach, is probably due to the change of position of the liver—*i. e.*, to the right, and to the early fixation of the duodenum. At first the colon has, in every part, quite an extensive *mesocolon*, but by degrees this shortens until its ascending and descending portions are so closely bound in their respective lumbar fossæ that they lose their serous covering, except in front; not so with the transverse colon; its mesocolon is usually long enough to permit the colon to reach the body wall in front, and to lie just beneath the great border of the stomach. This length of the transverse mesocolon serves many purposes; it forms a roof to the entire bowel below the duodenum; it also gives great freedom of motion to the transverse colon, so that it can accommodate itself to the varied changes in the conditions of

the surrounding viscera. In cases of stricture of the rectum and consequent extreme distention of the large intestine with gas, the middle of the transverse colon will often descend to the pelvis, giving the entire colon an appearance not unlike the capital letter M. I am careful to repeat that the colon has its own transverse mesocolon, and that it does not depend upon the great omentum for it. The latter is asserted by some authors, much to their discredit. As the colon twists upon itself, and comes to lie across the duodenum, its mesocolon (Fig. 11) furnishes a fixed point for the commencement of the mesentery of the small intestines, which begins at the left border of the second lumbar vertebra and proceeds downward to the right iliac fossa. This base of the mesentery is scarcely longer than six inches, while its periphery, encircling the bowel, is nearly eight times the length of the body. The coiling of the small intestine is greatly facilitated by the looseness of its connection with its serous envelope. This is especially noticeable in fœtuses and young infants. In all such cases the entire small intestine can be drawn from its peritoneal sheath, leaving the mesentery unbroken save at the opening through which the gut is extracted. By the disposition of the small intestine within the borders of the colon a mutual benefit accrues. The small intestine is placed beneath the abdominal muscles, where every exertion will excite peristaltic motion, and therefore aid in carrying its contents to the colon. By the same muscles the same pressure through the small intestines is exerted upon the colon, which cannot escape its influence, and as its contents cannot revert to the small intestine they are carried forward to their destination.

It is interesting to examine the various portions of the alimentary canal with reference to their relation to the serous covering. Thus, from the œsophagus, it has been stripped off and appropriated by the lungs and heart. The latter had need of it as organs of ceaseless activity; while the œsophagus, being but a mere conduit for articles of alimentation, was better off without such a covering; but with the stomach it is far otherwise: this is more than a receptacle for food; it must have capacity for liquids, taken often with great rapidity and in great quantity. To be equal to all the emergencies that the stomach may be called upon to meet, the great omentum is formed (Fig. 8, B, C, D). This permits large bloodvessels to course along both curvatures of the stomach, a circumstance only present in this organ, and make a vascular network about it, which, in case of thirst, will abstract water from the stomach with wonderful rapidity. The disappearance of covering from the transverse duodenum, the formation of the mesentery and the mesocolon, have already been touched upon.

It remains briefly to speak of the portal circulation. The blood to the digestive tract comes from the abdominal aorta from three principal vessels, viz., the cœliac axis, the superior and inferior mesenteric arteries. Now, the aorta lies *behind* the peritoneum in its entire course, and consequently receives no covering from it; not so with the three large branches to the digestive tract. These, as well as all the blood from the digestive tract to the liver, lie throughout their entire course between two layers of serous membrane. The cœliac axis (Fig. 12) supplies the liver, stomach, pancreas, spleen, and duodenum, but none of its vessels pass in *front of*

the stomach to reach their destination. A peculiarity in the venous blood from the digestive tract is, that it, like the arteries, passes behind the stomach. If the mesenteric vein had passed beneath the duodenum, it would have been obliterated; if it had passed in front of the pancreas, the weight of the omentum would have dragged upon it; had it gone in front of the stomach,

FIG. 12.

Diagrammatic. Introduced to show the course of the venous blood of the digestive apparatus to the liver.

then a full stomach or acts at vomiting would have interfered with its function; in its course to reach the portal vein it passes between the pancreas and duodenum, and under the stomach; the portal vein thus comes to lie anterior to the foramen of Winslow; but neither it nor the hepatic arteries have any hand in creating this foramen, as stated in at least one prominent text-book.[1]

[1] For a description of the formation of the tunica vaginalis and descent of the testes, as related to the peritoneum, see Annals of Anatomy and Surgery, Nov. 1883.

[After the reading of the preceding paper:—]

Dr. CHAPMAN said: I have listened with much interest to the paper of Dr. Allis. There are several facts which confirm what he has said about the disposition of the peritoneum. Examining the abdomen of an adult, it seems that the disposition of the peritoneum is as given in Gray, but that is not so. If the abdomen of a fœtus at eight months or term is opened, the peritoneum will be found as described by Dr. Allis.

If you take a net, making one sack, and spread it over a number of objects on a table, representing the liver, kidneys, and the other abdominal organs, and then invaginate the net, we shall have a representation of the greater and smaller sack of the peritoneum. There is then no difficulty in placing the net over the different objects, as the peritoneum is disposed over the viscera.

In the body of the fœtus, you can raise the sack and demonstrate that it consists of four layers. By inserting a pipe in the foramen of Winslow, the sack may be distended. If the sack is raised, you find the transverse colon covered as it is in this drawing. In the adult, the sack becomes adherent to the covering of the transverse colon, and we then have five layers, instead of four as shown in Gray.

In almost all the mammalia (at least of several hundred that I have examined), I find the peritoneum disposed as here represented. The transitory stage through which the peritoneum passes in the human animal, is retained as a permanent condition in the lower animals. This is true, not only of the peritoneum, but of the other organs of the body. In the other mammals, this sack can be lifted, leaving the transverse colon covered with peritoneum. The only exceptions that I have found, have been in the gorilla, the chimpanzee, and ourang. In all these animals, the transverse colon adheres to the greater sack, as it does in man. It, therefore, seems that the facts of comparative anatomy, and the facts of embryology thoroughly bear out what has been said by Dr. Allis. It is no longer a question of theory, but it is exactly what is represented here.

There are one or two other points in connection with the development of the peritoneum that may be mentioned. The peritoneal and pleural cavities may be regarded as simply large lymphatic sacks. If milk is thrown into the peritoneal sack, in a few hours the lym-

phatics of the diaphragm will be filled and the fluid may even pass to those of the pleura. This will serve to explain the fact why you may coincidently find the peritoneal cavity filled with fluid or pus, and at the same time the pleural cavity. The peritoneum is nothing but a large dilated lymphatic sack placed around the abdominal organs.

In regard to the development of the pleura, Dr. Allis begins by saying that there is one general cavity. It seems to me that it would be better to consider the pleura as consisting of two sacks in the beginning, and that the lungs are thrust between these, thus producing the same thing as represented here. This will account for there being no communication between the two. There is one reason for not considering the pericardium as a part of the pleura, and that is, the fact that the phrenic nerve runs between the pericardium and pleura.

Another point as regards the peritoneum is one which is of interest to surgeons, and that is, that the viscera being pushed forward, carrying the peritoneum before them, can have no peritoneum behind. Hence in hernia occurring between the quadratus lumborum and latissimus dorsi muscles, there will be no peritoneal covering to the bowel.

Dr. W. H. Pancoast: I agree with Dr. Allis in his demonstration of the development of the peritoneum. While really the diagram in the text-book of Gray may not be correct, yet practically it is correct. When these different layers become adherent, they practically constitute one layer and they may be demonstrated as one layer.

I may add one interesting fact. Year after year in making this demonstration, I have represented the layers of the peritoneum by a mosquito-netting, and this has shown that the ordinarily described arrangement of the peritoneum is not correct. You cannot take a membrane like that and take the different viscera and place them in the proper position and demonstrate the great omentum at the same time. You must pin up the netting to represent the omentum.

I might mention one point in regard to the pericardium. We find that this membrane is firmly attached to the diaphragm and extends upward, surrounding the heart and passing up over the great vessels and mingles with the deep fascia. In violent exercise, as in athletes, a full inspiration precedes the effort and this might seriously affect the heart, if it were not for this support passing through the cavity of the thorax and to the deep fascia of the neck.

CONGENITAL MONOCULAR IRIDEREMIA.

By

GEORGE C. HARLAN, M.D.

[Exhibited April 1, 1885.]

I wish to show a specimen of a rare form of congenital anomaly of the eye, absence of the iris. I removed this eye this afternoon. There is an entire absence of the iris, and a displacement of the lens. The lens is cataractous, and seems to have undergone calcareous degeneration. It was floating in the vitreous humor, and could be seen only when the patient leaned forward. Three years ago, I exhibited a case of congenital absence of the irides, and called attention to the fact that it was almost always binocular. So far as I know, there is only one case on record in which the defect existed on only one side.

THE DISPOSAL OF SEWAGE, AND THE PROTECTION OF STREAMS USED AS SOURCES OF WATER SUPPLY.

By

GEORGE E. WARING, JR.,

NEWPORT, R. I.

[Read by invitation, January 6, 1886.]

One of the most important questions now claiming attention, in connection with the sanitary condition of houses and towns, relates to the manner of getting rid of the foul constituents of the liquid effluent in such a way as to prevent annoyance from odors, danger from the development of infection, and the contamination of water-courses and harbors. The difficulties presented have always existed, and the ill effects of ordinary methods have always been known. It is only since attention has been called to the relation of the foul effluent of house and town drainage to the spread of diseases that the difficulty has been generally recognized, and only since the modern growth of the larger towns that its dangers have been fully apprehended.

In a few fortunate cases, such as those of towns discharging their effluent into great rivers like the Mississippi, the remedy is provided by the volume of the stream to which the sewage is added. In London, the Thames has become so foul that the Royal Commission

which recently investigated the matter, pronounced the system of outflow there established to be "a disgrace to the metropolis and to civilization." In Paris, the discharge of the great collecting sewers into the Seine has produced a condition that is no longer tolerable. Were the houses and streets of Philadelphia drained in accordance with modern ideas of decency and efficiency, the condition of the Delaware would perhaps become little better than that of the Schuylkill. The great addition to the tidal volume of the harbor of New York, due to the inpouring through the East River of the higher tide of Long Island Sound, mitigates, but does not remedy the foul condition due to the discharge of the city's wastes along its whole border. In short, all considerable communities in this country and in Europe are confronted to-day by a problem which it is absolutely necessary to solve. In one way or another, the waste organic matter of our domestic life and of our industries must be withheld from the waters into which our sewers discharge.

I propose to notice, only in passing, what may be called the more artificial means for accomplishing this result; that is, the chemical and mechanical methods of precipitation, decantation, and distillation. There are instances in Great Britain and on the Continent of Europe, where the difficulty of securing a sufficient area of land for what we may regard as the more natural treatment is so great, that there is no choice but to resort to these more costly and less efficient processes.

It is possible, by an addition of the salts of iron and other salts, of milk of lime, of comminuted clay, etc., to cause the deposition of nearly or quite all of the suspended impurities of sewage. The deposit or sludge

thus formed is sometimes treated by filtration, drying, filter-pressing, etc., in such a manner as to become available as manure for use on lands not too far removed.

In connection with the Liernur system of pneumatic removal in Holland, an ingenious plan has been devised by which the solid parts of the slightly diluted effluent are recovered by evaporation *in vacuo*. In Germany, other more elaborate processes, including the distillation of the purified effluent, have been measurably successful in the production of the salts of ammonia for commercial use, and of a residuum of some manurial value.

It seems hardly worth while, in the short space here at my disposal, to do more than to refer to these processes, which, however important under favoring circumstances, are not suited to conditions prevailing almost universally in this country. There is probably no town in the United States where any treatment of sewage is desirable, where it will not be cheaper and more simple to overcome the difficulty by the aid of surface or subsurface irrigation or by more concentrated filtration—filtration, in greater or less degree, being an essential element of all irrigation. This application to the soil is not only cheaper and more simple, but is also more effective. No chemical or mechanical treatment has thus far been devised which produces an effluent so entirely free from organic impurity and from the lower forms of life as that which is produced by properly regulated agricultural disposal.

We have been, until recently, quite in the dark as to the processes by which organic wastes are destroyed after application to the ground. We seem now to have some positive knowledge on the subject—knowledge due largely to the investigations of Dr. Frankland in

England, and of Schloesing and Muntz in Paris—these investigations being an application of knowledge derived from the biological investigations of late years.

It may now be accepted as a demonstrated fact, that the various processes of oxidation and nitrification by which organic impurities in the soil are reduced to their mineral elements or to elementary salts, are due to, or are largely aided by, the reproduction and growth of bacterial life. This was especially well demonstrated in the Paris experiments, by the fact that, while sewage filtered through a suitable soil confined in a cylinder two metres high, the liquid being applied with intervening periods for aëration, was deprived of its impurities and allowed to pass out at the bottom of the column as pure water, the process being accompanied by a great increase of bacterial growth; the impregnation of the soil with the fumes of chloroform was sufficient, by arresting bacterial activity, to allow the sewage to be discharged as impure as it was received. Ample confirmation of the obvious conclusion was found in the fact that after the chloroform had entirely escaped from the pores of the soil the purifying effect was fully restored.

Roughly speaking, or rather practically speaking, the process is this: When sewage is applied to the soil its impurities are filtered out and are attached to, or involved with the particles of the soil. The water thus purified descends to the lower strata—to the underdrains or other means of outlet. As the water descends, fresh volumes of air enter and furnish oxygen, which it is the office and the means of life of the *bacterium termo* to combine with the retained impurities. The resultant product is fully decomposed matter available for the use

of plants or for innocuous removal in solution. There is much in the way of detail concerning the processes involved yet to be discovered, and investigation is active in this direction. Enough is now known to constitute the basis of a rational theory applicable to the practical processes we are considering. These processes have long been employed with more or less completeness and with corresponding perfect or imperfect results.

It would be a fair summing up of the whole case to say that we now know, by well-established theory and by ample practice extending over more than twenty years, that we have in the soil a universally available agent for the safe, inoffensive, and complete destruction of everything in the way of organic waste that we may deliver to it in a proper manner.

As we look over the field of practice, beginning with the Craigentinney Meadows at Edinburgh, which have been irrigated with sewage for more than a century, and ending with the most complete modern examples, we find the greatest conceivable variety of conditions and a great variety of results. There are more than one hundred sewage farms in England to which are applied the effluent of as many towns, large and small. Some of them have been carefully arranged and are fed by sewers which deliver the sewage to them in a very early stage of decomposition, and where the great bulk of the storm water is diverted to other outlets. On these the best results are obtained. In other cases the attempt is made to purify the whole outflow, storm water and all, with the natural result that floods of storm water deluge the land at those times when it is already drenched by the heavy downfall and is incapable of

absorbing more water. Here the result is far from satisfactory.

In Germany, sewage irrigation farms connected with the city of Berlin, with Dantzic, and with Breslau, arranged in accordance with modern ideas on the subject, are all instances of notable success.

At Gennevilliers, where about one-fifth of the whole outflow of the sewers of Paris, about one-half of the dry-weather midsummer flow, is distributed over a very large area of thirsty and hitherto almost valueless sand and gravel, the result is excellent.

In our own country, we have as yet no instance, save the recent one of Pullman, Illinois, of a general system of sewage irrigation. The result there, according to all reports, is as good as it has been elsewhere.

I have recently had an opportunity of carrying out a rather complete system of sewage disposal in your near vicinity, in connection with the State Asylum for the Insane at Norristown. As I have endeavored there to work according to the best indications of modern experience, and as my efforts have been freely sustained by the Trustees of the Asylum, I know of no way in which I can better set forth what seems to me the best method for disposing of sewage than by describing, somewhat in detail, the work that has there been done and the result thus far achieved.

The Asylum is situated on high land in the north-eastern suburbs of the city, on the water-shed of Stony Creek. The buildings include eight large pavilions, with the necessary administration buildings, kitchens, laundries, etc. The population of the institution is now not far from 1600. The consumption of water is not far from 150 gallons per head, daily, and all manner of

organic waste, except the garbage, which is fed to swine, must necessarily be disposed of through the system of house drains and sewers with which the institution is amply supplied.

As at first constructed, the system of drainage, delivering not only foul wastes but roof and surface water as well, discharged its contents through a large main sewer running in a southerly direction, entering a brook which delivers, in a short distance and with a steep incline, into Stony Creek near the hospital station of the railroad.

Although the ample facilities for the drainage of such an institution afforded by Stony Creek had been a chief inducement offered by the city of Norristown for the location there of the Asylum, the buildings had not long been occupied before complaints were made of the gross fouling of the creek due to the hospital drainage.

Following the erroneous idea which is so prevalent, that the chief source of defilement in sewage is fecal matter, two immense settling tanks were established in the grounds for the retention of these solids only, the effluent enriched by the products of their putrefaction, overflowing into the sewer and running into the creek. As urine and kitchen wastes are quite as bad in their ultimate condition as is fecal matter, and much more serious in amount, this process did not long satisfy the complainants and an injunction was threatened to prevent the discharge of any sewage from the hospital into the stream. In consequence of this threat, the authorities engaged me to devise and carry out some other system of relief. What has been done is as follows:

The old system of drains and sewers has been left as it was, to carry off roof-water and surface-water from

the courts, walks, etc., these finding their way into Stony Creek through the old channel. A separate system of sewers, six inches in diameter, has been constructed in connection with all of the foul wastepipes of the building, so that they carry all kitchen and laundry waste, bath and toilet waste, closet discharges, and dining-room and pantry sink waste, everything, in short, which contains foul refuse. For lack of sufficient appropriation some changes that it was thought desirable to make in the interior plumbing of the buildings were not made. It was attempted to substitute water-closets with traps for the untrapped closets already in use. These latter have their outlets connected with ventilation pipes in which a strong current of air is maintained by the use of steam—a very expensive process. They are also flushed by automatic cisterns in such a way as to consume a very large volume of water. The trapped closets with independent flushing cisterns, to be worked by the use of the seat, would have effected considerable economy; but it was found that the odor produced during their use, especially in the morning when used in rapid succession, could not be kept out of the wards without the introduction of a new system of apartment ventilation. This experiment was therefore abandoned as too costly in construction, and the old method of forced downward ventilation continues, in spite of the cost of its maintenance.

The dining-room sinks were subject to the usual difficulty from the accumulation of grease. Such sinks as are to be retained were supplied with a flushing outlet, which is accomplishing a most satisfactory result. The same system was applied on a very large scale to the kitchen sink, which is so arranged that its outlet is

ordinarily kept plugged. When discharged, from 50 to 100 gallons are delivered at a time through a 4-inch pipe; this sweeps all grease and other refuse rapidly forward and constitutes an effective flush for the kitchen drain, which formerly gave trouble even with a large grease trap and with the occasional introduction of a jet of steam.

The system of sewers described is brought together to a single main outlet 6 inches in diameter, constructed like the branch sewers of vitrified earthenware pipe. This main sewer delivers into one corner of an open tank 40 feet square and about 6 feet deep, built of brick laid in and coated with Portland cement, and having a Portland cement concrete floor. It is probably absolutely tight. Immediately in front of the inlet (the mouth of the main sewer) there is a vertical iron screen, with one-half inch openings, for holding back paper, rags, a small part of the fecal matter, and the miscellaneous rubbish delivered from the buildings. In the corner of the tank, diagonally opposite the inlet, there is a vertical Rogers' Field annular siphon, the overflow of its discharging limb, 8 inches in diameter, being 5 feet above the floor of the tank. About a foot below the overflow level, and through the south wall of the tank there is a 4-inch opening which is ordinarily kept plugged. At the bottom of the tank and connected with the main discharging chamber outside, there is a pipe through the wall closed by a screw-gate.

This flushtank, the largest I believe that has ever been constructed, has a capacity between its floor and its overflow point of 60,000 gallons. It occupies about two hours in discharging, and during this time about 15,000 gallons more flow in from the main sewer, so

that the total amount discharged at each operation averages about 75,000 gallons.

The discharging siphon is so arranged that when sewage begins to overflow at the top of its discharging limb its outlet becomes sealed, its contained air is soon withdrawn, and it discharges, full bore, until the tank becomes empty, when air is taken, first at the inner end and then at the outlet end; the siphon "breaks" and nothing more can be discharged until the overflow begins again. The discharging chamber delivers into an 8-inch pipe which leads to the irrigation field.

In the course of the main drain, above the tank, there is a gate by which the flow can be diverted from the tank and sent through a by-pass directly to the 8-inch pipe. Below the tank there is another gate by which its discharge may be diverted to a truck-patch near by whenever sewage is required for irrigation there. Higher up in the truck-patch there is a sewage carrier which is connected with the 4-inch hole near the top of the tank. By a proper adjustment of these different arrangements sewage can be sent from the buildings directly to the irrigation field without going through the flush-tank, or can in like manner be diverted to the lower carrier of the truck-patch. By opening the gate at the bottom of the tank sewage is allowed to flow out as it flows in, and can be sent in uniform moderate quantity to the irrigation field or to the truck-patch. By removing the plug near the top of the flushtank, the upper carrier of the truck-patch can be made to receive sewage for four or five hours, but as the entering stream is too large to be completely removed at this point, it in time reaches the overflow and starts the siphon, and the whole accumulation is discharged through this.

These truck-patch carriers, the by-pass and the various gates, are incidents of the system and are intended only for occasional use; ordinarily, the discharge is in a strong flow through the main 8-inch pipe connecting with the irrigation field.

Around the whole interior of the tank, near its top, there is a perforated brass pipe connected with the main water supply by a float-cock at the bottom of the tank. This sends a spray over the walls of the tank during about an hour, partly before the discharge is complete and partly during the earlier filling of the tank. Its purpose is to wash down accumulations of slime which might in hot weather become offensive. This part of the apparatus is to be removed during the winter season.

The irrigation field proper is about 1000 feet distant, and its upper side is $27\frac{1}{2}$ feet lower than the outlet of the flushtank. This field is separated from the hospital grounds by a public road and by Stony Creek. Its lowest side is nearest to the grounds, and its higher or further side is bounded by a mill-race a little above its highest level. The area of the field available for irrigation is about 12 acres. Lying to the north of it is a piece of waste land 2 or 3 acres in extent, which has been arranged for use as a relief area in emergencies. Along the upper side of the irrigation field bordering the mill-race, is a deep trench with sloping grassed sides and with a planked water-way at the bottom, for catching and carrying away the infiltration or overflow of the mill-race. Immediately inside of this ditch is the main sewage carrier, which extends from one end of the field to the other, and has a fall of 1 to 600 from its highest point, which is in midway of the field. At this highest point there is a circular well, 6

feet in diameter and 7 feet deep, measuring from the top of its wall, which is a little higher than the surface of the ground. The 8-inch discharge pipe delivers into the bottom of this well, which thus serves to check the velocity due to the rapid fall of the connecting sewer. At its top there are three semicircular openings two feet wide and one foot deep. One opens into the north carrier and one into the south. The third opens into another carrier which leads directly toward the middle of the field. Ordinarily, only one of these openings is used at a time, and one is sufficient to deliver the whole flow. The north and south carriers, the banks of which are lower toward the field than toward the mill-race, overflow with much uniformity and deliver the sewage at the top of a well-graded inclined surface over which it runs until absorbed. In no case does the sewage run quite the whole distance across the field. Movable gates being set at one point or another, the whole tankful of sewage may be delivered at pleasure over any area of from 2 to 4 acres, according to the inclination of the surface, and its condition of saturation by rain, or its dryness.

The carrier which runs toward the centre of the field delivers into a level ditch which surrounds a level tract about 2 acres in area. This is crossed by a series of parallel ditches 3 feet wide and 2 feet deep, separated by beds or banks 8 feet wide. These beds are to be used for the cultivation of vegetables, forage, osiers, or whatever may be thought most advantageous. They receive their sewage solely by lateral absorption.

This level tract constitutes a relief bed capable of receiving several tankfuls of sewage in succession, and

resembling somewhat the "intermittent filtration" beds used in England.

The whole of the field outside of the level tract is now sowed with rye and grass and is already well covered. It is intended to use it as grass land only, and with the relief that can at all times be afforded by the level tract, by the emergency field to the north, and by the truck-patch, it need never be overflooded, and the sewage may at any time be kept off from any part of it long enough for harvesting. Judging from the experience of similar fields in England, it will be necessary to crop it three or four times during the season.

A considerable element of the cost of the work was due to the very unfavorable character of the ground, which, like that of the whole neighborhood, is a very heavy, stony, argillaceous deposit, underlaid by a stratified limestone sometimes at a depth of 2 feet or more, sometimes lower than it was necessary to excavate. A sort of swale ran through the field from end to end, the land being higher at the bank of the creek than here. It was crossed with ditches, largely occupied by a tussock swamp due to a heavy underlying stratum of clay, and its old fence rows and ditch rows were overgrown with willows and other trees.

The cost of the preparation of the land was not less than $8000, including underdraining, grading, uprooting trees, etc.; that is to say, it cost this amount to put it into the condition of a reasonably well graded, cleared, and naturally well-drained field. That part of the cost would be avoided where land of proper character is available.

One item of the preparation consisted in the laying of over 20,000 feet of draining tile at a depth of from 5 to

8 feet, more than half of which required rock cutting from a few inches to 2 feet in depth. It should be said that the rock is so fractured that water easily finds its way down to the level of the tiles. The underdrains are generally 25 feet apart.

The surplus water of the soil was substantially all removed by the digging of the ditches, so that when the tiles had been laid and covered, during dry weather, they discharged a very small stream. It took a long time to get the stony filling of the underdraining ditches so compacted and solidified as to prevent the direct flow of water from the surface to the tiles. Even now, there are some voids which have not been detected and filled, and some water flows directly from the surface to the tiles during the application of sewage. It soon ceases and the effluent is clear within half an hour after the tank ceases discharging. In a short time this difficulty will be corrected and it will always be clear.

The above is a brief and rough description of the general arrangement of the work. Its operation, so far as observed, may be thus explained:

All of the water-closets in the wards are flushed out at intervals of 4 or 5 minutes with a copious discharge from automatic tanks. This flow runs directly to the new sewers and is sufficient in amount to maintain, day and night, a constant cleansing flow. The water-closets in the administration buildings and officers' quarters are operated by hand. They are all in direct communication with the same sewers. So, also, are the urinals (automatically flushed) and the wash-sinks and baths of the whole establishment; also, the sinks in the various dining halls. Such of these as are not to be abandoned on the completion of the new refectory build-

ings have been provided with flush-pots, by which their wastes are held back until accumulated to the amount of 6 or 7 gallons, and then they go forward, with a rush and in mass, to the sewers.

The whole series of kitchen sinks are connected with a similar apparatus, by which, as above stated, from 50 to 100 gallons at a time are discharged into the sewers through a 4-inch outlet. The laundry apparatus from time to time contributes its very copious flood to the volume of the sewage.

Probably, in no case, does more than 15 minutes elapse between the emptying of any vessel in the establishment and the arrival of its discharge at the flush-tank. Within certainly less than 12 hours and often less than 8 hours, the tank becoming full, discharges the whole accumulation into the main outlet-sewer leading to the irrigation field. The stream, running out through one or other of the carriers leading from the well, overflows its banks and spreads over the land. This complete process may take two and a half hours, so that if we add together the extreme limits of time, we have less than 15 hours between the discarding of waste matter and its application to the surface of the irrigation field.

It is safe to say that putrefaction exists nowhere, at any time, throughout the whole system, and there is never at any point the least suggestion of the putrid odor inseparable from common sewers and cesspools. The only element of the mass which, in its fresh condition, is malodorous is the fecal matter; as this is distributed through and drowned by not less than 2000 times its volume of water, it counts for nothing as a source of exhalation. The whole flow might be dis-

charged on the lawn in front of the administration building without offence save to the eye.

A few small coprolites withstand the rough usage of the current and are carried on to the ground, but they are so few and so very far between as not to be noticeable. Whatever solid matter passing through the screen is lodged on the surface of the field is destroyed by natural processes everywhere active. In no case is the amount of such matter or the effect from it worth noticing.

The real filth of the sewage—its dissolved and finely divided suspended matters—is carried into the ground, is retained there, and is destroyed by oxidation, and, through the activity of bacteria, by nitrification. As filth, it cannot pass through the soil nor very far into it. If the products of its resolution are not consumed by plants, they pass off with the underdrainage, as soluble salts devoid of all organic character and unaccompanied by the lower forms of organic life.

After a few days' use of a single tract, the sewage is turned to another tract and again to another and another, being allowed nowhere to run long enough for the closing of the ground against infiltration by clogging, or for the gorging of its interior spaces with impurities. In short, the process of purification is complete and continuous. Experience elsewhere indicates that the soil will in time improve in its purifying power from year to year for a long time.

There are larger examples of the purification of sewage by irrigation elsewhere in the world, and examples of which the lesson is enforced by long experience; there is, so far as I know, no example in existence more carefully arranged as to its details, involving the

overcoming of greater natural difficulties, or better illustrating the more modern technical methods of the art.

I may be excused for suggesting that this example, so near to your own doors, points out the way in which the sewage of the towns now draining into your own water supply may practically, and without too great cost, withhold their filth from their drainage, making the Schuylkill once again a fit source from which to draw household water.

Another method of disposal by application to the land which is especially applicable to isolated houses and to establishments, where the discharge cannot be removed and must be concealed, is what is known as "Subsurface Irrigation," a process invented by the Rev. Henry Moule, for use in connection with the earth-closet, and first applied systematically by Rogers Field, Esq., an English engineer, in connection with the drainage of some cottages at Shenfield, in Essex. Its use in England has never extended very much.

In this country its first application was in connection with my own house at Newport, in 1869. After ample experience and observation of its efficiency, I began to use it in my private practice as an engineer, in disposing of the sewage of isolated houses. In 1876 I had become so confident of its success that I applied it to the sewage of the whole village of Lenox, Mass. In 1879 it was applied on a still larger scale at the Woman's Prison, at Sherburne, Mass., and in 1881 to the hotel at Bryn Mawr. The details of this system have been very materially perfected, and its use is now common in many parts of the country, there being hundreds of examples in New England and probably as many within a radius of ten miles about Orange, N. J.

The Lenox work being the oldest of the larger ones and the one longest in use, may be taken as an illustration of the system generally.

Lenox was a scattering village with less than 1000 persons living in reach of the sewers. The fund available for sewage was small, not enough to lay an outlet-sewer to the river, over two miles distant, to say nothing about work in the town. Indeed, a discharge into the river would not long have been tolerated. At that time (1876) much less was known than now as to the efficiency of sewage irrigation. As the most promising means for overcoming the difficulty, I decided on the adoption of subsurface irrigation, using 10,000 feet of distribution pipes, underlying a well-graded area of about one and a half acres. The pipes were laid a little more than one foot below the surface. They were common 2-inch agricultural sole tiles laid directly on the earth. They were divided into 20 lines, with as many connections with the main pipe leading from the flushtank. The manner of connection was never very satisfactory, and the general arrangement was never entirely successful from the point of view of an expert. However, although the field was but a few hundred feet distant from the village, there was never any serious complaint from it, and there was generally great satisfaction with it, although, as the flushtank had no settling basin for holding back solids—only a strainer—there was always more or less trouble from obstructions, and, as the population increased, these obstructions increased, until now the whole affair is in such condition that it seems necessary to reconstruct parts of the work in accordance with methods since universally adopted. At the same time, with all its drawbacks, it has been essentially suc-

cessful and satisfactory, no nuisance having arisen from it that was perceptible at any distance from the field, and no attention having been called to it by reason of its condition. It has been much quoted and visited, as an instance of a great advance in the disposal of the sewage of a village, and it only needs slight improvements to make it available for perfect work for years to come, provision being made for distribution over the surface of the field, at times, during the short period when the village is full of visitors.

At the Woman's Prison the system was much more correctly constructed and has been correspondingly more successful, though it is seriously overtaxed with an effluent of about 30,000 gallons per day; the more especially as the contributing pipes lie in a bed of muck and heavy silt, one of the least successful materials for this use.

At the Bryn Mawr Hotel the same system has always worked satisfactorily since it has been sufficiently extended to deal with the large volume of sewage; but, mechanically considered, this is not a test case, for all of the sewage is received and retained in large cesspools, the absorption drains taking care only of the putrid liquid discharged from them.

After large experience with this method of distribution, I should not hesitate to use it for a community of any size if it were a mere question of mechanical arrangement and of purification. I should hesitate to use it except where the distribution ground is in the immediate vicinity of houses, simply because it is much more costly and much less simple than a discharge over the surface which, as has been amply proven before and is

amply proven now at Norristown, answers every requirement of simplicity, safety, and decency.

I cannot better close this paper than with extracts from a report made on the 25th day of July, 1885, to the Chamber of Deputies of France by M. Bourneville, a deputy, submitted in the name of the committee appointed to examine the proposed law having for its object the agricultural utilization of the sewage of Paris and the purification of the Seine.

I adhere as closely as possible to the original text, thinking that, as former statements about this work have been disputed, a close translation is more important than a freer rendering in English. M. Bourneville says:

"The vast experiment at Gennevilliers comes in its turn to confirm the great laws of natural purification and agricultural restitution, attested by the numerous examples that we have collated.

"It was in May, 1869, that is, sixteen years ago, that the sewage first reached the land of the plain of Gennevilliers. There had already been for two years (1867 to 1869) several thousand cubic metres distributed by irrigation or treated by chemical reagents at Clichy on an experimental field, where the pumps now stand; a certain number of vegetable products had been obtained on about two-thirds of an acre. The experiment transported to the other side of the Seine, at the beginning of the plain of Gennevilliers, began in 1869 on six hectares (fifteen acres) bought by the city of Paris and retroceded by it to several well-disposed cultivators. The disasters of the war came and destroyed the first installations; they were put into condition again at the commencement of 1872 and since then the service has been

regularly performed. On the 3d of June your commission visited in detail the pumping station of the city of Paris and the plain of Gennevilliers. We have gathered together, on the ground and in the documents placed at our disposal, the most circumstantial information on the results obtained."

The report then describes the character and arrangement of the pumps, connecting pipes, distribution pipes, etc., and continues:

"The volume of sewage sent into the plain of Gennevilliers, which was only 1,765,621 cubic metres in 1876, was 15,000,000 in 1880, and finally 22,493,992 cubic metres in 1884. From 1872 to 1885 there have been spread on the plain of Gennevilliers 157,000,000 cubic metres.

"The irrigated surface has undergone a corresponding development. Beginning with 57 hectares in 1872, it reached 121 hectares in 1874, 200 hectares in 1875, 450 hectares in 1880, and finally 616 hectares on the 1st of January, 1885. The sewage is distributed over the tracts by about 20 laborers, each of whom is charged with the supply of from 25 to 30 hectares and with the management of about 30 outlets. In view of the extreme division of the property and the diversity of culture, the volume of sewage delivered into the plain is distributed so uniformly as to require no reservoir and no 'regulator;' simple standpipes placed near the steam pumps and at different points along the pipes regulate the pressure.

"During the season of active vegetation, the cultivators are present on the fields during nearly the whole day to the number of about 1500 men, women, and children; they lead the water into the ditches from the

distribution outlets with a care and a skill which would leave nothing to be desired in the best irrigations of the south of France. During the three or four months of winter, vegetation is only partial; the laborers then interfere more directly; they cause the sewage to flow in the gutters and trenches in such a way as to insure purification by oxidizing action; the solid portions remain in the gutters and form a paste which the peasants afterward incorporate with the earth in the first plowing of spring. This is the case especially for cereals; the vegetable products utilize the winter deposits in the form of a top-dressing of the beds.

"At Paris, as at Berlin, this formation of the deposits and the irrigation continue during the greatest cold, sewage water having always a temperature of at least 5° or 6° (40° to 44° Fahr.) This was realized in the severe winter of 1879 and during the three weeks of continuous frost of last winter. During great floods of the Seine the pumps are generally stopped, leaving to its flow the removal of the entire discharge of the main sewers."

Then follows a table, showing that in 1884 the volume of sewage used in irrigating, per month, varied from 1,205,358 cubic metres in February, to 2,766,782 cubic metres in July. The monthly average for the year was 1,874,491 cubic metres.

"At the time of the visit of your Committee, the volume delivered to the cultivators each day reached from 130,000 to 140,000 cubic metres for every twenty-four hours. More than one-half of the sewage of Paris was purified and utilized by the plain of Gennevilliers during the heated term and during low water of the

Seine. That is to say, at the time when the discharge of sewage into the Seine is specially objectionable.

"The results obtained in the plain, from the point of view of cultivation, are most remarkable. Your Commission traversed, during more than two hours, fields covered with products of the greatest variety and abundance; vegetables of all sorts, cereals, grass, and nurseries.

"They obtain generally 20,000 to 40,000 head of cabbage per hectare, 60,000 heads of artichokes, 10,000 kilogrammes (over 100 tons) of feeding beets, and five or six cuts, yielding from 80 to 100 tons of green forage. The gross product obtained per hectare varies from 3000 to 10,000 francs ($600 to $2000) and even more for crops.

"It has been said, in the presence of the Commission, that the horticultural products of Gennevilliers were of bad quality. It has been written that *the vegetables produced by this soil, surcharged with infected water, are bad to the taste, and the forage offered to live stock is not nutritive, and is, besides, rejected by them.* On this point here is the opinion formulated in a special report to the Agricultural Society of France by M. Michelin."

Then follows a table showing that the 616 hectares were occupied for the growth of cabbage, artichokes, potatoes, asparagus, salads of various sorts, peas, carrots, beans, parsley, onions, beets, luzerne, grass, sundry vegetables, nursery stock, trees, and cereals. M. Michelin says:

"The Society has, through its committees, always observed the results obtained in the horticultural experiments which have shed light on this question, which we in the horticultural world of Paris regard as solved from the practical point of view, with reference to the beauty

of the products, their quality as to taste, the success of the production and the certainty of sale. In affirming the quality of the vegetables to be proper for the nutrition of men as well as of animals, it should be explained that the liquid ought not to be put in contact with those parts of the plant which are above ground, but only with their roots."

The Committee asserts that "all the vegetables of Gennevilliers are advantageously sold in the *Halles*, as well as in the markets of the Environs. They carry off the first prizes at the horticultural exhibitions of Paris, and even of Seine-et-Oise. About 800 cows are fed with the aid of the irrigated grass and plants. The average dose of sewage used by a hectare, divided over the whole surface dedicated to irrigation, is about 40,000 cubic metres a year. It is really, if we include what is not used directly, about 50,000 cubic metres. Certain parcels, specially treated, under an arrangement with the cultivators and by way of experiment, with high doses, have been receiving for three years 80,000 cubic metres by regular irrigation summer and winter. They are covered with a luxuriant vegetation.[1]

"The rental value of land which was formerly from 90 to 150 francs a hectare ($7.20 to $12 per acre)—we speak, of course, of the cultivated land—is now from 450 to 500 francs ($36 to $40 per acre) in all the irrigated area. As to the selling value, it is from 10,000 to 12,000 francs ($800 to $960 per acre). All leases now accepted by the cultivators carry the provision that the high rent is not consented to, except on the condition of sewage being disposable for the leased land.

[1] At this rate, one acre would purify the sewage of over 500 persons (at 40 gallons per day).

"The commune of Gennevilliers asked and obtained, by the treaty of 1881, that for a period of twelve years the sewage should remain at the disposition of its inhabitants as freely as they should desire, whatever might be the projects and works of the city of Paris for the extension of irrigation (elsewhere).

"Irrigation with sewage has, therefore, brought wealth to Gennevilliers. Notwithstanding the evidence of these results, there is among the adversaries of the present project one who maintains that this wealth is an illusion, and that in reality the irrigation has caused to Gennevilliers an irreparable wrong, because no one seeks this locality for the construction of villas. The answer is simple; we take it from M. Francisque Sarcey: 'The population, which was not dense,' says he, 'cultivated more or less well a rebellious soil. They had only to scratch the ground to meet the sterile sand and the arid gravel. A few country houses had pushed in here and there around Gennevilliers itself; but it was by exception, *for the emigration of the Parisian Bourgeoisie in search of villas passed to one side and pushed generally further on.* Those who had stopped there could have been seduced only by the cheapness of the land. It seemed that this country, struck with a sort of malediction, was never to lift itself from this condition, when, in 1869, the sewer commission of Paris selected it as the theatre of an experiment which was to produce a happy change in its appearance.'

"The purity of the subsoil water, which receives all of the water filtering from the irrigated land is perfect. M. Pasteur has testified to this with his high authority before the Committee.

"All may judge of this as your Committee has done

by the examination and the tasting of the water that flows out of the 5 lines of drains 18 inches in diameter which surround the village of Gennevilliers and discharge into the Seine about 1 kilometre from each other. These drains, having a total length of about 8 kilometres (5 miles) have been established at a depth of 4 metres (12 feet) at the normal level to which it was desired to reduce the subsoil water; in the case of floods, or of very heavy irrigation, these drains facilitate the outflow of the water and prevent the invasion of quarries and cellars.

"As M. Marie-Davy, Director of the Observatory of Montsouris, testified before the Committee, the water of the drains is chemically pure. It contains barely 0.001 of a gramme of organic nitrogen to the cubic metre even at those points, as in the experimental basins of the gardens belonging to the city of Paris, where the annual or continuous dose reaches and passes 80,000 cubic metres per annum. With Liebig's boullion which shows 62 micro-germs in a cubic centimetre of water of the Vanne, 1410 for the Seine at Bercy and 20,000 for the sewage, there are found only a dozen inoffensive micro-germs in the water of the drains, which thus sustains the opinion of M. Pasteur. At the same time, the large content of chlorine, 0.07 of a gramme per litre, indicates the presence in the subsoil water of a large proportion of purified sewage which has passed through the ground.

"The sanitary condition of the commune of Gennevilliers leaves nothing to be desired; the Mayor and his Adjuncts, Doctors Thobois and Cornilleau, testified before the Committee at its visit to Gennevilliers, and it is enough to walk about in the plain and see the

vigor and good health of the hundreds of men, women, and children who are working eagerly [avec ardeur] in the midst of the irrigated fields to understand the true state of the case. The *general mortality* in 1865 was 32 per 1000. In 1876 and 1881 it was only 25 and 22. No epidemic of typhoid fever has existed for long years, although the irrigations were continued on a large scale during the cruel epidemic which attacked Paris in 1882. Not a single case of cholera occurred in 1884. Never from 1869 to this day, although the inhabitants eat their own vegetables, even uncooked, has there been observed a single case of *anthrax* or *septicæmia*. In fact, all of the information that we have gathered from most of the physicians who have had occasion to be called to Gennevilliers proves that intermittent fever shows itself very rarely, and that the number of cases does not exceed that of localities more or less remote, and of which the fields are not subjected to irrigation.

Still another argument pleads in favor of the excellent sanitary condition of Gennevilliers: that is, the increasing growth of population as shown by the following table:

1st of January, 1869	. . . . .	2186 inhabitants.
" " " 1872	. . . . .	2218 "
" " " 1880	. . . . .	2389 "
" " " 1885	. . . . .	3245 "

"Such are the facts that your committee has established in the plain of Gennevilliers; it has been constantly accompanied by the authorized representatives of the population of the plain and its suburbs: MM. Pommier, Mayor, and Retrou, Adjunct, of Gennevilliers, Berthou, Mayor of Saint Ouen, Hennape, Mayor of Puteaux, our friend M. Bailly, Mayor of Courbevoie,

Honorary Inspector-General of Public Assistance. A deputation of the cultivators of the plain gave the committee all the information as to details that it needed. No discordance was developed; the unanimity was complete concerning the excellence of the results obtained and the absolute innocuity of the system. The majority of your commission cannot refrain from expressing to you the confidence that these demonstrations give it in proposing to you the continuation and extension of sewage irrigation."

The publication of this report must have been most gratifying to M. Durand-Claye, the champion, and the wise and eager director of the work at Gennevilliers, who has fought its battles against prejudice, ignorance, and malice from the days of its struggling infancy to this hour of its complete triumph, and his own.

At Gennevilliers as at Croyden, Berlin, Dantzic, Breslau, and the Norristown Asylum, complete evidence is set before us of the absolute efficiency of the system of purification by application to the soil, which, it seems to me, on the score of economy and of completeness, as well as by reason of the conditions generally prevailing in this country, has such advantages over the best of the chemical systems that it is, in at least a very large majority of cases, better suited to our needs.

Nor can it be doubted that this system will enable us to restore and to maintain the purity of our water-courses, especially when these are used as the source of water for domestic use.

ON THE NUTRITIVE VALUE OF SOME BEEF EXTRACTS: AN EXPERIMENTAL INQUIRY.

By

THOS. J. MAYS, M.D.

[Read February 3, 1886.]

During the last seven months my leisure time has been principally employed in efforts to determine the nutritive value of some of our principal beef preparations, and I beg your brief attention this evening for the purpose of bringing the results of these researches before you.

That there is no idea so erroneous as to be wholly devoid of truth, nor one so true as to be wholly devoid of error, is an ancient maxim, and its truth is probably as well illustrated in the prevailing ideas of the nutritive value of the beef extracts as it is in anything else. A study of the extensive literature of the beef extracts shows the inconstant and indefinite opinions which have been held concerning their action, and also illustrates the fluctuations of thought which the medical profession is liable to undergo. Liebig, who was one of the first to invest this question with scientific interest, held no less than three different theories regarding their action, during the last twenty years of his life. In his *Letters on Chemistry*, published in 1851, he distinctly rates the

beef extracts as nutriments—*i. e.*, as substances which are capable of supplying working force to the muscles of the body. In *Auerbach's Volkskalender*, page 148, published in 1868, and in his *Chemische Briefe*, issued in 1865, he expresses the opinion that they are merely condiments (Genussmittel), and, hence, only act as stimulants to the process of digestion, and to the general nutrition of the body. Later, he conceived the idea that they are nutrients not only in the sense of supplying force to the body, but as furnishing material wherewith the bodily tissues are constructed. Since his death, however, medical opinion has by almost universal consent reverted to the second idea entertained by him, viz., that the beef extracts are of no or very little value as foods. It is true that this has been questioned by some whose clinical observations have led them to different conclusions, yet I do not know of a single work on physiology, therapeutics, or pharmacology, that does not assign the beef extracts among the non-nutritious alimentary agents. Probably the most positive expression of this feeling among those who are considered modern authority on such subjects is that of Dr. Fothergill in his *Handbook of Treatment, or Principles of Therapeutics*, who, on page 537, says that, "as a food, beef-tea ranks low. It contains meat-salts, a small quantity of albumen, and a little gelatine, together with some advanced nitrogenized matters useless in histogenesis. But there is little in it to repair tissues, and less in it to sustain life, so far as our knowledge yet extends. There is little real force-bearing material in the protean compounds of beef-tea. For the starving fever patient, to give him beef-tea alone is almost to give him a stone when he asks for bread. It makes him

feel better for the time being, but that is due to its stimulant properties."

There can be no doubt that the cause of the prevailing scepticism concerning the nutritive value of beef extracts is largely due to the experiments which were made by feeding animals exclusively on these preparations, with the result that all of them died within a short period of time. Indeed, Kemmerich affirms that they died more quickly than those which were left to starve from hunger. This has the semblance of proof that beef extracts are not capable of supporting life. Sober reflection teaches, however, that no animal can subsist continuously on any single food, and that such a test would unceremoniously refute the food value of any substance no matter how nutritive it might be. But, beset with difficulties as this investigation evidently is, the question is not whether these substances are capable of sustaining life alone, but whether it can be shown that they contain any nutritive value at all, and if so how much. Here everything depends on the method which is employed to determine this question. It is imperative that this should be definite and exact. It must be able to show the functional state and condition of the organism, before, during, and after the addition of these substances; or, in other words, it must demonstrate whether the organism is, or is not capable of performing work when these substances are added to it.

To meet all these desiderata I selected the isolated frog's heart, which has already proved itself so pregnant with good results in the hands of Ludwig, Cyon, Kronecker, Bowditch, and others, in the full belief that it will show itself as capable here as it has in clearing up other problems in experimental physiology. Only quite

recently Prof. Yeo (*Journal of Physiology*, vol. 6, No. 3, p. 93) has succeeded in demonstrating with it the reduction of oxyhæmoglobin, a phenomenon the existence of which was assumed, but never proven by direct observation.

The experiences of Prof. Kronecker, Drs. Martius, McGuire, Von Ott, and others, show very clearly that when the frog's heart is well washed out with a 0.6 per cent. saline solution and then allowed to beat with the same, its pulsations gradually get less in force and in elevation, until at last in the course of an hour or two it becomes entirely exhausted and is unable to work any longer. But when this stage of complete fatigue is reached, and the heart is filled with blood or serum, it recommences to beat, and its pulsations gradually gather in strength and in elevation until their former altitude is attained. If, instead with blood, the heart were refilled with the saline, or any alkaline, acid, or even alkaloid solution, it would never show any sign of returning vitality. This observation means that the saline, alkaline, acid, and alkaloid solutions are devoid of material with which the heart can perform its function, and, although it works while it is filled with these solutions, it does so at the expense of the nutritive material stored up in its own meshes, and not with any energy derived from these solutions. Indeed, Dr. Pohl-Pincus (see *Verhandlungen der Berliner Physiologischen Gesellschaft*, Feb. 23, 1883) has brought forward evidence which indicates that in the wall of the frog's heart there exist lacunæ (Nahrspalten) designed for the purpose of storing up nutritive material. As soon as this stored-up supply is exhausted, it ceases to beat, and any solution which is now applied to it with a view to reëstablish its pulsations must con-

tain some elements which are capable of nourishing it. These essentials are found in the blood and serum of most animals. Dr. Von Ott (*Archiv für Physiologie*, March, 1883) has also shown that milk has the power of nourishing the heart. And Prof. Ringer (*Journal of Physiology*, vol. vi. No. 6), in following the same line of inquiry, found that both albumen and gelatine are capable of sustaining the heart's contraction. In my own work on the nutritive value of different concentrations of blood, performed in the Berlin Physiological Laboratory under the direction of Prof. Kronecker (*pub. Verhandlungen der Berliner Physiologischen Gesellschaft*, Jan. 1883), I found that the amount of work of the frog's heart performed with blood depends entirely on the degree of dilution of the latter agent. Dr. McGuire found, however, that 1 : 3—*i. e.*, one part of blood to three parts of saline solution—gave the best results.

In summing up the literature upon the action of the frog's heart in its relation to our subject, we find the following: That the heart in performing work consumes oxygen; that after it is once completely exhausted, it cannot resume its pulsations unless food energy is supplied to it from the outside; that blood, serum, milk, albumen, and gelatine are capable of acting as foods and of restoring its power of contraction, but no other substances have, heretofore, been shown to possess this property; that the pulse elevations and the amount of work which the heart is able to do, depend in a great measure on the degree of concentration of the food. These data, moreover, demonstrate very fully that the heart does not exercise the function of a purely circulatory organ in these experiments, but that it is an organic medium possessed

of the power of assimilation, of transformation of energy, of contraction, and of respiration.

The apparatus which was employed in testing the food value of the beef extracts was the following: The heart, by means of a double canula, on which it is securely but gently tied, is attached to a Kronecker-Bowditch heart apparatus, and then through a mercury manometer, and an appropriate glass needle, it records its pulsations upon a revolving cylinder. The heart is fed or transfused through the double canula from two Mariotte's bottles, with any desirable fluid; and in order to keep it pulsating regularly, minimum shocks of one Daniell's cell through a DuBois sliding induction coil, were employed about every four seconds. Before testing the beef extracts, the heart was in every instance well washed out with a 0.6 per cent. saline solution, after which it was allowed to work with the same until all its stored-up material was exhausted and its pulsations reduced to zero. Then a weak solution of the inorganic elements of the beef extracts, chiefly containing phosphate and carbonate of potash, and chloride of sodium, was carefully tested on the heart to find out whether it had any power to induce cardiac contractions, but it failed in every instance. The same was also found to be true of a solution of urea whenever it was tried. After this preliminary work a number of different dilutions of a beef extract previously prepared with a 0.6 per cent. saline solution (usually in the following proportions—1 : 100; 1 : 250; 1 : 500; 1 : 666; 1 : 1000; 1 : 2000; 1 : 4000) were alternately introduced into the heart and their effects noted. The stronger dilutions, like 1 : 100 to 1 : 500, had no influence whatever in restoring cardiac contractions; but in every specimen which was examined,

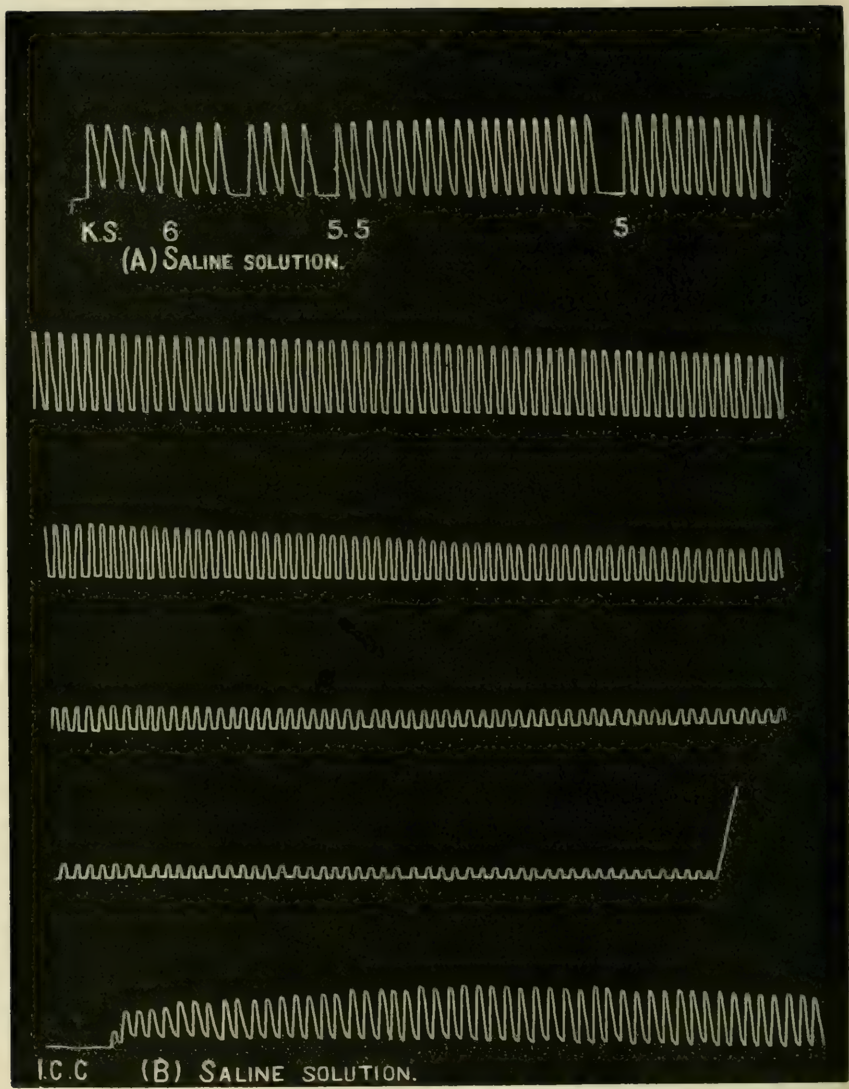
the dilutions from 1 : 666 to 1 : 2000 had the power of reviving and of maintaining the beats of the heart, as the following tracings given by Valentine's Meat Juice clearly show.

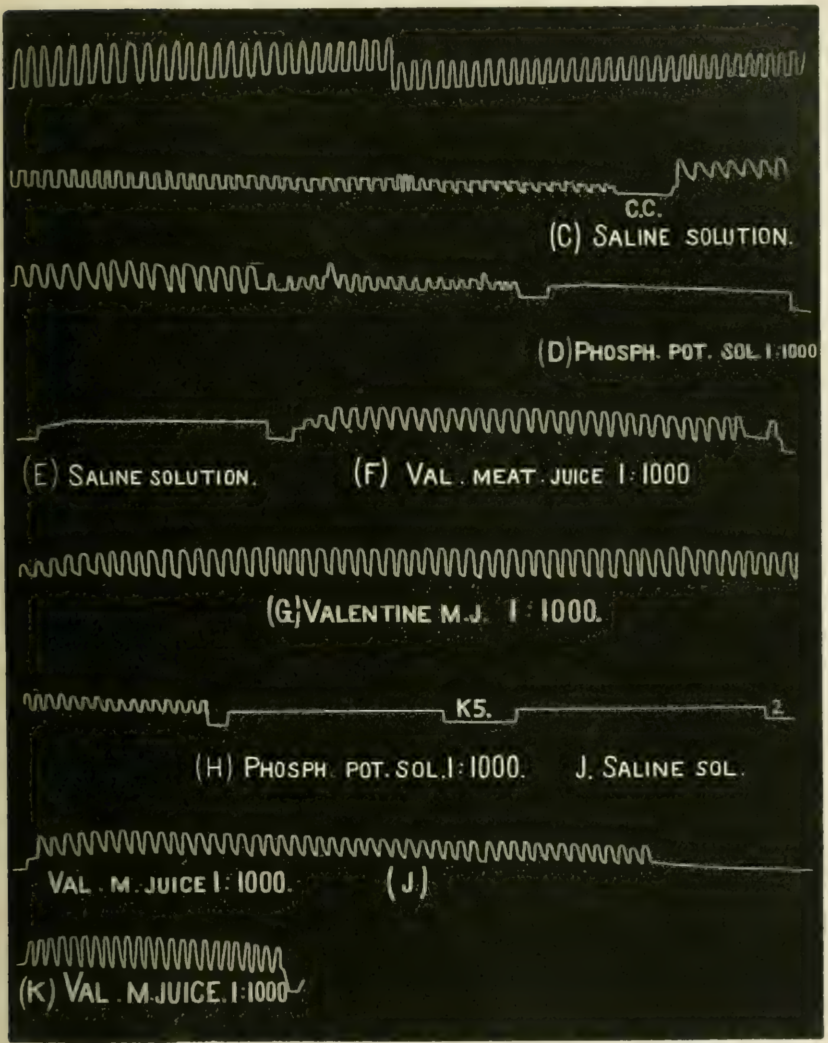
In this experiment the heart was filled with a 0.6 per cent. saline solution (A) and allowed to work with it until it was fatigued, at (B) and (C) this was repeated, and the pulse elevations grew after each filling, not because the saline solution conveyed any nutriment to the heart, but because it had not consumed all the energy stored up in its walls. At (D) and (E) neither the phosphate of potash nor the saline solution could revive the heart, showing that its whole store of energy was exhausted. At (F) and (G) Valentine's Meat Juice was introduced and the pulsations grew to about half their original height. The other beef preparations which were examined in the same manner as Valentine's Meat Juice, were Reed & Carnrick's Beef Peptonoids; Parke, Davis & Co.'s Sarco-peptones; Johnston's Fluid Beef; Cibil's and Liebig's Extracts, besides milk, and all gave evidence that they possess nutritive properties. Although the amount of nutrition varies somewhat in each specimen, as will appear later on, every one gave tracings substantially similar to those given by Valentine's Meat Juice.

Judging from these data, I think the following deductions can be made concerning the influence of the beef extracts on the frog's heart:

1. That they are absorbed and assimilated.

2. That they contain material which has the power of inducing muscular contraction—a power which has heretofore only been shown experimentally to exist in the higher animal albumens or proteids.


K.S. 6
5.5
5
(A) Saline solution.
I.C.C
(B) Saline solution.



EXPLANATION OF THE TRACINGS.

(To be read from top to bottom, and from left to right.)

(A) Heart filled with 0.6 per cent. saline solution; (B) refilled with same; (C) refilled with same; (D) filled with phosphate of potash solution, 1 : 1000; (E) filled with saline solution; (F) filled with Valentine's Meat Juice, 1 : 1000; (G) refilled with same; (H) filled with phosphate of potash solution, 1 : 1000; (I) filled with saline solution; (J) filled with Valentine's Meat Juice, 1 : 1000; (K) refilled with same.

3. That, hence, whatever else they may be, they are nutrients in the full implication of that term.

After it was thus demonstrated that these beef preparations contained definite nutritive properties, it was deemed desirable to ascertain the value of each, and means to this end were instituted by comparing their effects with those of a two per cent. solution of dried bullock's blood alternately on the frog's heart in the following manner: In the first place the heart, after being washed out, was filled with the two per cent. blood solution and then allowed to beat until its pulsations were reduced to a minimum, or until the whole nutritive supply of the blood solution was consumed; after which it was washed out again and filled with a solution of the beef preparation to be tested, and allowed to beat with it until its pulsations were again reduced to a minimum. A large number of comparative tests were made of each of the above named beef preparations in this way, and the following products were obtained, which indicate the mean percentage of the number of pulse-beats given by each preparation, while that of blood is taken as 100. These figures are probably not absolutely true, but they give an approximate idea of the nutritive worth of these extracts when compared with that of a two[1] per cent. blood solution, which is capable of producing a normal cardiac contraction.

	Mean Percentage of Number of Pulse-beats.
Liebig's Extract of Beef	58
Johnston's Fluid Beef	59
Valentine's Meat Juice	60
Cibil's Extract of Beef	61
Sarco-peptones (Parke, Davis & Co.)	62
Beef Peptonoids (Reed & Carnrick)	74
Milk	100
Two per cent. solution of dried bullock's blood	100

[1] Two per cent. solution of dried bullock's blood gives as good cardiac contractions as fresh blood in proportion of 1 : 3.

From this table, it appears that all these preparations contain very nearly the same amount of nutritive material except the Beef Peptonoids, which contain from ten to fifteen per cent. more than the others. It must not be forgotten in this estimation that the Beef Peptonoids are not a pure beef extract like the rest, but a compound of the latter with milk and gluten. Therefore, in order to get at the true value of this preparation it is important to test each ingredient. Milk alone gives as good results as the blood solution, as can be seen from the table, and there is no doubt that a portion of its valuable property is due to this agent.

The next question which arises in this investigation is as to which of the many organic bodies resident in the beef extracts this nutritive property is due, and I must confess that principally owing to the difficulty of obtaining these organic extractives this portion of the work remains incomplete, but I am making preparations to resume it at an early day. It can be safely stated, however, that the inorganic elements of the beef extracts contribute no share to this result, for the phosphate of potash solution contains all these, and in every instance where this was tested it failed to bring out the least cardiac reaction. Hence, these can be left out of consideration.

When finely divided beef muscle is exposed to the action of about four or five times its own weight of cold water for four or five hours and then well pressed out, it loses from sixteen to twenty-four per cent. of its original weight. In this watery solution is contained from two to fourteen per cent. of flesh albumen, while the remainder is made up of kreatin, kreatinin, sarkosin, sarkin, xanthin, carnin, inosit, fat, glycogen, and the inorganic elements ;

while in the residue there is left nothing but fibrous tissue, principally composed of muscular fibres and connective tissue, which is tasteless, rejected by animals, and entirely unfit for nutrition. Therefore, it is very probable that all the nutritive element of beef muscle resides in its organic extractives.

Now there cannot be the least doubt that the variable amount of albumen contained in the beef extracts furnishes some of the nutritive property displayed by them, but it is far from my intention to claim that this is exclusively due to it. Indeed, I have found altogether unexpected indications during the investigation which may, on further examination, throw some more light on this question. One thing, however, remains steadfast throughout, and that is, if the albumen of flesh is not the sole nutritive element in these beef preparations, then some or all of the organic bodies which they contain must be nutritious, and can no longer be regarded as effete products of the animal body.

It is not necessary to point out, then, that the multitudinous composition of these extracts makes them a very valuable class of alimentary substances, both for nutritive and constructive purposes; and before closing this interesting subject, let me refer very briefly to the practicability of the subcutaneous introduction of these agents. I think it is quite evident now that these preparations are assimilated and utilized by the frog's heart without previously passing through a digestive process, and we have no reason for believing that this does not also obtain in the human organism; hence in conditions where the stomach has an intolerance of food, or where there is any hindrance to the introduction of the same through the primary passages the object of feeding the

patient can readily be secured by introducing a suitable preparation hypodermically. A number of years ago I treated several such patients with Valentine's Meat Juice, which, on account of its complete solution in the normal state, is probably preferable for this purpose. I injected from fifteen to twenty minims three times a day with good effects. I failed, however, to keep a record of these cases, but recollect that one was a case of persistent vomiting caused by a severe blow on the head, and that after each injection the patient expressed himself as being stronger and feeling as if he had eaten something. Not the least irritation was produced at the point of injection, and I think this field deserves further investigation.

In conclusion, I wish to thank Dr. Marshall, of the University of Penna., and Dr. Leffmann, of Jefferson Medical College, for valuable aid in prosecuting these researches.

THE MECHANISM OF INDIRECT FRACTURES OF THE SKULL.

By

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[Read February 3, 1886.]

Historical Study.

The earliest authentic mention of a fracture of the cranium occurring at another point than that upon which the violence which caused it was applied, is to be found in the writings of Hippocrates, who lived about 500 years before the Christian era. To call him a surgeon may seem startling to some who have thought of him only as the author of the oldest medical work now extant; but that he was a surgeon of great sense and acuteness his writings abundantly testify. Nor should it give offence to any that he should be claimed for the fellowship of surgeons, since, as Justamond has pointed out, in one of his admirable essays,[1] the practice of surgery must have antedated that of physic; for, as he remarks: "In the primary ages of mankind, when the most perfect of all created beings had yet scarce degenerated from that state of perfection in which he was first produced from the hands of the Creator, disease

[1] Outlines of the History of Surgery. Surgical Tracts of the late J. O. Justamond, F R.S. etc. London, 1789.

was yet unknown upon the earth. Man had then no wants but such as the neighboring stream or the labor of his own hands would supply, no cares but those of a domestic nature, amply compensated by the satisfaction which attends them. His mind was not yet weakened by intemperance, nor his body impaired by debauchery; exercise was his only physic, and unbroken, undisturbed rest his only restorer. But, even in those happy and tranquil times, man was not exempt from the consequences of accidental violence. His body was not less exposed to common casualties and to a variety of strokes that might bruise or wound the flesh, or dislocate or fracture his bones. He might be torn by the fangs of some wild beast, or affected by the bite of some venomous insect. Such were the first and most natural evils to which man, in the place assigned him in the order of created beings, must have been obnoxious, and therefore his thoughts would necessarily be engaged in finding out some means of relief for these accidents. Thus, from the nature of the subject, as well as from the testimony of Celsus and many other remote authors, it appears that surgery was incontestably the most ancient branch of medicine, the parent of all the rest."

Hippocrates, then, the father of surgery, let us say, gives the earliest definition of the fractures we are about to study: § 8. Ὀστέον τιτρώσκεται ἄλλη τῆς κεφαλῆς, ἢ ᾗ το ἕλκος ἔχει ὤνθρωπος, κὰι τὸ ὀςτέον ἐψιλώθη τῆς σαρκός, etc.[1] (The bone is broken in another part of the head than that in which the man received the injury, and the bone was stripped of the flesh.)

This definition remained unimproved until more than

[1] Hippocrates. Œuvres Complètes d'Hippocrate, par E. Littré. 10 vols. 8vo. Paris, 1841. Vol. iii. p. 211.

two thousand years later, when, in 1873, Félizet suggested the division of fractures of the cranium into "immediate and mediate;"[1] if, indeed, this suggestion be an improvement.

Hippocrates despaired of the cure of fractures remote from the seat of the wound, and seems to have had no more definite idea about them than that they might occur.

The next author, to whose writings access may still be had, who refers to this subject, is Celsus, who lived about the beginning of the Christian era. Celsus says: "Solet etiam evenire, ut altera parte fuerit ictus, et os alterâ fiderit." (It sometimes happens, also, that the blow has fallen upon one point, and the bone is cleft in another.) He furthermore recommends cutting down upon any point where there is softening and swelling, in case no fissure is found at the point struck, and bad symptoms arise.[2] Here was a decided advance upon the teaching of Hippocrates, another being the observation of Celsus that the vessels of the brain might be ruptured without fracture of the cranium, anticipating a much later explanation of the term "*contrecoup*."

Soranus[3] (97–117 A.D.) says: "Resonantia secundum aliquos est facta in calvariæ partibus fractura oppositis iis quæ percussæ fuerunt, sine vulnere superpositum."

Oribasius (about 350 A.D.) says: "Ceterum quæ ab ipsis dicitur resonantia, rima quædam est, quæ non quo fuit ictus, sed adverso loco sit, simili ratione ac in vasis

[1] Félizet, G. Recherches anatomiques et expérimentales sur les fractures du crâne. Paris, 1873. P. 160.

[2] Celsus Aur. Corn. Celsi de medicina libri octo, cum notis integris . . . cura et studio Th. J. ab Almeloveen, etc. 8vo. pp. 749. Ludg. Batav., 1746. Lib. viii. De calvaria fracta.

[3] Soranus. De fracturarum signis, etc. Florentiæ, 1754. Pp. 46 and 47.

fictilibus, quæ altera parte percussa, in alteram rimam accipiunt."

It is said in many of the more recent works on counterstroke that Paul of Ægina (about 500 A. D.) denied the occurrence of the fractures at a distant point. For this denial, however, he offered an explanation which was considered wise when it was suggested as original about 1200 years later, namely, that the fractures attributed to counterstroke are chargeable to other blows, received after the first: by a fall, for example.[1]

The teachings of these great men were but little modified in many centuries.

In 1535 Berengarius discussed most intelligently the possibility of a fracture in an opposite part of the skull without lesion of the part receiving the blow. After going over the ground pretty thoroughly and asserting the ease and frequency with which multiple fractures, and fractures caused at the opposite side by a fall after the blow has been received, may be, and have been, misinterpreted, he concludes that he cannot deny that a true, independent, opposite fracture may occur.[2]

The idea of counterstroke was, so far as my investigations go, first plainly and unequivocally defended by Paw, who, in 1616, says that a blow may fracture not only another part of the skull, but even an opposite one; thus the forehead being struck, and remaining intact, the occiput may be fissured, or when one tem-

[1] Paulus Ægineta. The Seven Books of Paulus Ægineta. Translated by Francis Adams. Sydenham Soc. London, 1846, 3 vols. Book vi., Sect. xc.

[2] Tractatus perutilis et completus de Fractura Cranii, ab eximio artium et medicinæ Doctore D. Magistro Jacobo Berengario Carpensis, etc. 8vo. folii iiii. cx. Venetiis, MDXXXV., capitulum ii. ff. x–xiii.

poral or parietal bone has received the blow, the opposite one may give way.[1] He also notes a case in which he found, at the autopsy, that the outer plate of the bone which was struck remained unbroken, while a fragment was detached from the inner one and driven into the brain.[2]

In 1649, Guillemeau expressed doubt as to the occurrence of fractures by counterstroke, which, he says, many writers speak of. "Mais," he says, "telle fracture n'est iamais venuë à ma cognoissance & ne me puis persuader qu'elle puisse aduenir, si ce n'est quand les sutures sont serrëes & vnies ensemble, ou perduës depuis l'os frappé, iusques à celuy qui se trouue à l'opposite fracturé."[3]

In the same century Boerhaave recognized the fact that the brain may be lacerated or compressed, though the skull remains entire, and Van Swieten, in commenting on this statement, explains the phenomenon as due to the momentum communicated to the brain, which dashes it against the resisting interior of the opposite side of the skull.[4] Van Swieten also called attention to the possible injury of the diploë alone, without fracture of either table of the skull.[5]

Still in the seventeenth century, Stalpart van der Wiel speaks most unequivocally of lesions of the cranial contents at a point opposite to that which has

[1] Petri Paaw Amstelodamensis succenturiatus anatomicus continens commentaria in Hippocratem, de capitis vulneribus, etc. Lugduni Batavorum, Ann. MDCXVI.

[2] Ibid., p. 108.

[3] Jacques Guillemeau. Les Œuvres de Chirurgie. Folio. Rouen, 1649. De la contrafente aux os de la teste, p. 656.

[4] Van Swieten. Commentaries on Boerhaave's Aphorisms. Translation. 18 vols. 8vo. Edinburgh, 1776. Vol. ii. p. 399.

[5] Ibid., p. 339.

received a blow, giving this as a justification for trepanning.[1]

In 1708, Wagner published a thesis on contrecoup in which he quotes Fabricius de Aquapendente (1537–1619) as explaining it by the efforts of the air, which he supposed the skull to contain, to escape from the side opposite to that on which the blow was received. Marcus Marci, he says, attributed it to the spheroidal shape of the skull and the special violence of the blow.[2]

In 1764, Aurran published a paper to show that the location of a fracture by contrecoup could be certainly determined. In this paper he calls attention to the fact that the sutures of the skull do not set a limit to a fracture. He claims that the temporal regions are most liable to fracture, partly because they are comparatively thin, and partly because of their architectural peculiarities, since they embrace the parietals and are driven asunder by blows which fall upon the vault of the cranium.[3]

Soon after this, occurred a veritable epoch in the history of our subject. The Royal Academy of Surgery of Paris, which had been founded in 1631, proposed in 1760, and again in 1765, as the subject for a prize, the following proposition: "To establish the theory of counterstroke in lesions of the head; and the practical conclusions which may be drawn from it." On the first of these occasions the prize was not awarded; on the

[1] Stalpart van der Wiel. Observ. rariorum . . . Centuria prior. 16mo. Lugduni Batavorum, 1687. Observ. VII. and comments on it.

[2] Wagner. Sur les Contrecoups. Haller's Collection de Thèses. 5 vols., 12mo. Paris, 1757. Tome i. pp. 11–28.

[3] Aurran. Observations sur les contre-coups, qui tendent à prouver qu'on peut découvrir sûrement l'endroit fracturé, selon le lieu qui a reçu le choc. Journ. de Méd., Chir., et Pharmacie, Paris, 1764, tome xxi. pp. 252–260.

second, it was awarded to Grima,[1] who sent in for the second time the same essay which he had sent in for the former competition, and which had been thought the best at that time.[2] The same question was proposed for the prize of 1768, when prizes were awarded to Saucerotte[3] and Sabouraut,[4] for which Chopart[5] also presented an essay, which was accepted and published by the Academy. Chopart not only recognized the elasticity of the skull, but exaggerated it, and, as a diagram in his essay shows, believed that a blow produced alternating and reciprocal shortening and lengthening of the axes, in a manner which some authors describe as undulatory.

Nor would the history of this epoch be complete without mention of a valuable memoir upon counterstroke in other parts than the head,[6] written by David, though published under the name of his pupil, Bazille—to which a double prize was awarded in 1771.

All of these writers considered the mechanism of fractures by counterstroke critically. Grima defined contrecoup as an injury inflicted at any point other than that to which the violence was applied: fracture of the cranium at an opposite point—which was often spoken of as if it were the only true counterstroke—

[1] Louis. Recueil d'observations d'anatomie et de chirurgie pour servir de base à la théorie des lésions de la tête par contre-coup, etc. 12mo. pp. 385. Paris, 1788. Pp. 13, 37.

[2] Grima. Memoires sur les sujets proposés pour le Prix de l'Academie royale de Chirurgie. Tome iv. Paris, 1778. Pp. 246–273.

[3] Saucerotte. Ibid., pp. 368–438. [4] Sabouraut. Ibid., pp. 335–518.

[5] Chopart. Ibid., pp. 519–562.

[6] Bazille. Mem. sur la proposition suivante; Exposer les effets des contrecoups dans les differentes parties du corps, autres que la tête, etc. Prix de l'Academie royale de Chirurgie. Tome iv. pp. 563–633.

This essay was translated and published in English in 1789, in the surgical tracts of the late J. O. Justamond, F. R. S., etc., edited by Mr. William Houlston—4to., London, 1789.

being only one form of it; and he included under his definition injuries of the contents of the cranium without fracture of the bony part. All this, it will be seen from what I have stated above, had been more or less imperfectly laid down long before; but it was not until the epoch of which I am now speaking that it was clearly formulated. Grima insisted upon considering the skull as one piece after the sutures had closed, a very important matter in regard to the mechanism of fractures in it. He also calls attention to the possibility that a fracture attributed to counterstroke has been caused by a fall after a blow, a point in regard to which, as we have seen, Paul of Ægina referred 1200 years before. Grima compared fracture of the cranium by *contrecoup* to that which occurs in a stone-wall, which, when struck at one point, may break at another. He divides contrecoup into seven kinds, one being that in which the contents of the skull alone are lacerated. He speaks of Valleriola as having recognized a *contrecoup* by the occurrence of a gangrene at the side opposite to that which had received the blow, and of Bartholini as having drawn the same conclusion from the presence of an abscess. Grima, finally, advises trepanning at the suspected point, when symptoms arise which cannot be accounted for by the local injury, claiming that if it proves useless, it can, at least, do no harm.

Saucerotte adopted practically the same definition of contrecoup as did Grima: "A lesion produced by a blow in another place than that which received the blow." He makes eight kinds of counterstroke, one of them involving the diploë alone. In explaining the mechanism of counterstroke, he considers the skull as a solid case, which, in its vertical section, presents the

appearance of a scalene triangle. He dwells upon the varying degrees of thickness and strength in its different parts, and claims that its fractures are due to overcurvation and to flattening. In this he foreshadows a theory of which I shall speak hereafter, as well as in the further statement that when the skull is struck it "undulates" and changes its shape "alternatively." The vibration theory, involved in comparisons of the skull to a wall, as done by Grima, or to a glass or clay vase, which was a very ancient comparison, he rejects. He lays stress on the very important point that the skull contains fluid contents which are extremely incompressible, holding that a blow sets in motion a column which is practically solid and which strikes with great violence the opposite side—a conclusion which contains a fatal error, since the laws of hydrostatics make it clear that such a force is transmitted equally in all directions. Finally, he directs attention to a point which should never be overlooked, namely, a due appreciation of the character and direction of the force which effects the injury. Among his practical conclusions, one is to trepan more boldly than some surgeons think advisable nowadays, and another is to cut into the brain itself in certain cases.

Sabouraut speaks of the skull as a spheroidal case which cannot break without changing its shape. Under the influence of a blow, he says, its diameters lengthen and shorten, and shorten and lengthen alternately—a statement which would appear to be inverted unless it be understood to refer to the diameter perpendicular to the direction of the blow, in which case it fits exactly the theory already alluded to as to be spoken of hereafter. He explains the physics of the skull, as he under-

stands them, quite thoroughly, and acknowledges the difficulty of making a practical application of the information which an understanding of them might be expected to afford.

Chopart describes the skull as an elliptical case, and divides *contrecoup* into eight varieties, similar to those already referred to. He also calls attention to the fractures produced by falls upon the buttocks, knees, or feet, and classes them under the heading of counterstroke.

The essay of David, mentioned together with those we have been citing, is interesting as showing the extent to which in those days the idea of counterstroke was stretched, for it includes a number of fractures which are nowadays attributed to muscular action, as well as injury of the contents of the pelvis, abdomen, and thorax by the concussion due to falls.

In 1773, Mehée de la Touche published a valuable monograph on counterstroke[1] in which the same broad definition of it is given, namely, "a lesion produced by a blow at another point than that which was struck." The opposite point he considers the most frequent seat of counterstroke, that occurring elsewhere he attributes to special weakness of the part which yields. From the consideration of counterstroke he goes on to that of its symptoms, and is led into an interesting discussion of cross-paralysis, in regard to which he records some instructive experiments on dogs. This subject had attracted attention since the earliest times of medicine, and had been very sagaciously studied in the beginning

[1] Mehée de la Touche. Traité des lésions de la tête par contre-coup. Small 8vo. pp. xix. 264. Meaux, 1773.

of the 18th century by Parfour de Petit,[1] whose experiments on dogs were of the most interesting and ingenious character.

It was not until the beginning of the present century that I find any noteworthy reference to the mechanism of fractures of the cranium in England, when Sir Charles Bell[2] compared the movements of the cranium under the influence of a blow to those of a bell, and illustrated his views by an experiment in which he placed two balls inside of, and touching a hoop, one at each pole of the direction of the blow to be applied, and two more outside of, and touching it at the poles of the transverse diameter. On striking the hoop, the two balls in the line of the force are driven toward the centre, while the two balls at the poles of the transverse diameter are driven away from it. This shows that a blow shortens the diameter parallel to the direction of the force and lengthens that one perpendicular to it—another foreshadowing of the theory already referred to and to be described hereafter.

In 1828, Mr., afterward Sir, Benjamin Brodie[3] accepted the current theory of counterstroke, and described the fractures caused by the impact of other bones on the cranium.

In 1844, Aran published an exceedingly valuable paper on fractures of the base of the skull,[4] in which he

[1] Petit. Nouveau Systême du Cerveau. Extrait des Lettres d'un Médecin des Hôpitaux du Roi à Namur (Parfour de Petit.) In Louis: Recueil d'observations d'anatomie et de chirurgie pour servir de base à la théorie des lésions de la tête par contre-coup, etc. 12mo. pp. 385. Paris, 1788.

[2] Sir Charles Bell. Surgical Observations. 2 vols. 8vo. Edinburgh, 1816. Vol. i. pp. 461–489.

[3] Brodie, B. C., F.R.S. Pathological and Surgical Observations relating to Injuries of the Brain. Medico-Chirurgical Transactions, 1828, vol. xiv. pp. 325–423.

[4] Aran, F. A. Recherches sur les fractures de la base du crâne. Arch. gén. de Méd., 4 sér., tome vi. pp. 180–209, 309–347, Paris, 1844.

formulated a theory which has been known by his name ever since. This theory was, that fractures of the base are always connected with fractures of the vault, from which they radiate, following a line within the zone in which the former occurred, and taking a course which corresponds with shortest route to the base. His conclusions are formulated as follows:

1. He never saw a fracture of the base, caused by a blow, without a direct fracture.

2. Fractures of the vault joined the base by "irradiation," and crossed any sutures in their way.

3. They followed the shortest route to the base (the curve of shortest radius).

4. They were ordinarily limited to certain regions (corresponding to the several great fossæ of the cranium), and followed a certain direction.

5. Fractures of the vault sometimes coincide with *independent* fractures of the base; but only when there is considerable comminution (*ébranlement*) and very multiplied fractures.

These conclusions of Aran rested upon the results of a large number of experiments, upon fresh and dry skulls, by blows with very heavy hammers, and by throwing them down upon a hard pavement, as well as experiments in which he pitched entire bodies down upon their heads after having made the legs rigid by binding them to a strong iron bar. Certain writers have assumed that his experiments numbered one hundred. The only ground which I can find for this assumption is his statement that indirect fractures of the base by irradiation constitute 99 per cent. of the whole number—a statement which furnishes no clear indication as to the number of his experiments.

In 1847, Guthrie[1] published a monograph on injuries of the head, which is unsurpassed in the English language, in which he gives an excellent *résumé* of the history of various sorts of fractures of the cranium. Among them he speaks of counterstroke, the occurrence of which from temporal to temporal, or from parietal to parietal, he doubts, but says that fracture of the base from blows on the vertex or occiput are among the commonest accidents of surgery. Except for an uninterpreted reference to a statement of Chopart (*q. v.*), he expresses no opinion whatever as to their mechanism.

In 1853 was published, in *Guy's Hospital Reports*, a first instalment of Mr. Hilton's sagacious lectures on the cranium,[2] which was followed, in 1855, by their publication entire in a separate volume, by Dr. Pavy, after their revision by Mr. Hilton himself.[3]

In these lectures Mr. Hilton explains fractures of the skull upon the "vibration-theory," and speaks of the posterior clinoid processes and the extremities of the petrous bone as points to which vibrations are conducted, and where they are transmitted to the cerebro-spinal fluid, thus being "interrupted or lost before reaching the cerebral tissues," etc. The fallacy of this conclusion I shall have to consider hereafter, citing Mr. Hilton's opinions here for their historical interest.

In 1854 the keynote to a new theory of indirect

[1] Guthrie, G. J. On Injuries of the Head, etc. 4to. pp. vi. 155. London, 1847.

[2] Hilton, John. Notes on the Development and Design of Certain Portions of the Cranium. Four Lectures by John Hilton, F.R.S. Collected by F. W. Pavy, M.B. Guy's Hospital Reports, 1853, pp. 357–400.

[3] Hilton. Notes on Some of the Developmental and Functional Relations of Certain Portions of the Cranium. Selected by Frederick William Pavy, M.D., London, from the Lectures on Anatomy, delivered at Guy's Hospital, by John Hilton, F.R.S. 8vo. pp. 93, with 9 plates. London, 1855.

fractures of the skull—which had, however, as we have seen, been foreshadowed much earlier by certain observers—was struck in Germany by experiments made by Bruns,[1] of Tübingen, to determine the elasticity of the cranium.

In 1858, Mr. Prescott Hewett published a series of lectures on the anatomy, injuries, and diseases of the head, in which is contained an exceedingly full and able discussion of fractures of the cranium. He thought fractures by *contrecoup* to be rare, though he says that "indirect fractures" of the base are very common. As to the general law which governs the location and extent of fractures of the cranium, he accepts the views of Aran.[2]

In 1864, Chauvel published a thesis on fractures of the cranium,[3] founded upon a careful study of its anatomy, in which he laid great stress upon the elasticity of the skull, and described the "vibrations" produced by a blow, as the repeated and alternating shortening of the prime axis and lengthening of the transverse axis. He called attention to the fact that the petrous bone, although the hardest in the cranium, is the one most frequently fractured. He described three kinds of fractures: 1, direct; 2, contra-direct; 3, indirect. The first kind is simple enough. The second occurs opposite to the point struck, and he offered no explanation for it, except the comparison of fractures caused by the spinal column to the manner in which the handle is driven into the head

[1] Bruns. Die chirurgischen Krankheiten und Verletzungen des Gehirns, etc. Tübingen, 1854.

[2] Hewett, Prescott. Lectures on the Anatomy, Injuries, and Diseases of the Head. Med. Times and Gazette, 1858, vol. xvii. pp. 311, 312.

[3] Chauvel, F. Essai sur les fractures du crâne. Thèse de Paris, 1864.

of a hammer by striking the remote end of the handle against some resisting body. The third form of fracture he defined as that occurring between the point struck and that directly opposite. This he found in 11 out of 60 fractures.

In 1872, Schwartz analyzed 115 cases, in most of which the history was very incomplete, and he found fracture of the base alone, without fissure of the convexity, only 7 times. He found about 39 per cent. of fractures limited to the zones described by Aran and Hewett; while in over 60 per cent. this regularity was wanting. [Hewett's figures were about the same, though his conclusions were different.] This is one of the most important features of his investigations. Another is that a study of the fractures he had analyzed showed that the petrous bone was fractured in 66 out of 115 cases; and that force applied to the side of the head caused fractures somewhat parallel to the axis of the bone, while force applied to the occiput, or forehead, or vertex, caused fractures transverse to the axis.[1] This was an important contribution to the accumulating material for a new consideration of the mechanism of indirect fractures of the skull, as we shall see hereafter.

In 1873, Félizet published an elaborate illustrated monograph on the subject of fractures of the cranium, which, with the paper of Aran, may be regarded as among the most important contributions to the literature of the subject up to that time.[2] He regards this as a solid case, of a spheroidal shape, but claims that its internal

[1] Schwartz, Arnold. Zur statistik der Fracturen der Schädelbasis. In. Diss. 8vo. pp. 52. Dorpat, 1872.

[2] Félizet, G. Recherches anatomiques et expérimentales sur les fractures du crâne. 8vo. pp. 167. Paris, 1873.

configuration renders inaccurate a comparison of it to any geometrical figure whatever. By experiment he demonstrated that the dura mater makes the skull where it is attached more resistant to fractures caused by flattening its curve. Experimenting with a billiard ball let fall from a height, he found that its equator was increased, that this increase was greater in a direction at right angles to its fibres than in a direction parallel to them. The result was an ellipsoidal equator, and when a fracture occurred, this crossed the long diameter at right angles (p. 72). He unwittingly paraphrases the theory to which I have several times alluded, and which I shall soon consider, in saying, "We know that a fracture results from depression of the curved surface situated between two resisting pieces, and we know, also, that every effort tending to depress this curve divides into two forces, the one a driving force (*force de tassement*), which acts on the axis, or near to it, of the supporting walls; the other, a disruptive force (*force de glissement*), which acts transversely to them, and tends to separate the two extremities of these pieces" (p. 108). Again (p. 139), he compares the separation of sutures, the result of blows, to that which is effected in anatomical preparations by filling the skull with dry peas, and then soaking them, so that by swelling they tear the skull apart. He declares that there is a perfect analogy between this and what he calls the "*grand fracas*" (grand smash), in which he says "the bones, compressed over a large surface, rock (*basculent*), and this rocking movement sets up violently a dragging force perpendicular to the plane of the surface of union of the bones of the cranium." As we shall see hereafter, this is the same as saying that a force which depresses an elastic curved figure, will

enlarge its diameter, and be converted into a disruptive force acting at right angles (perpendicular) to the meridian. Herein Félizet comes near to the theory which the Germans call the "Berstungs" theory, although he does not quite reach it, and may perhaps, even now, not see his statements to be so strong a confirmation of it as they appear to me to be.

The term "*contrecoup*" Félizet rejects, and would adopt that of M. Beau, of Brest, who called indirect fractures "mediate." Félizet compares the mechanism of these fractures to those produced by a blow upon a wedge or a chisel. The part broken, he holds, is directly struck by another part, which, in its turn, received a blow and transmitted it. Yet he admits that, very rarely, these fractures by *contrecoup* do occur (p. 156). He divides fractures of the cranium into direct, indirect, and mixed. Indirect fractures include those which are independent. In respect to the application of the violence which causes them, fractures may be divided into immediate and mediate (pp. 159, 160). His conclusions may be briefly stated as follows: all fractures not immediately limited to the point of impact follow certain definite routes, to which they are restricted by the added strength of certain parts of the skull, which depends upon the presence of certain "*murs boutants*" (buttresses): the petrous bones, the orbito-sphenoid prominences, the occipital protuberance, and the naso-frontal protuberance. The basilar process and the antero-lateral parts of the occipital bone remain intact amid all fissures and constitute a "centre of resistance." The vibration theory, he says, is disproved by experiments, the fundamental phenomenon is a violent flattening of part of the vault, and a separation of the resisting portions (*pièces de résistance*)

which support it. When the violence is perpendicular to the surface of the cranium, fractures radiate directly to the base. In this, it will be seen, Félizet agrees with Aran. Finally, he admits that there are fractures, called by "*contrecoup*," the mechanism of which escapes us completely (pp. 161–164).

The opinions of Félizet are founded upon a careful analysis of a number of pathological specimens and upon a number of experiments upon fresh skulls. The value of his work can be appreciated only after a thorough study of it. The result of it was to bring him to a conclusion which may be succinctly stated as that indirect fractures are due to a disruption by splitting. There can be no doubt that the course of a fracture will be determined to a considerable extent by the reinforcements which the skull has, in certain places, from its buttresses (*murs boutants*); but, as we shall see hereafter, these very buttresses are often broken both transversely and longitudinally, and Félizet's centre of resistance is by no means a centre of immunity, but an exceedingly frequent seat of fracture.

A few years later—for events now crowd upon each other in the history of fractures of the cranium—Baum[1] opposed the vibration theory, and made some experiments by strewing sand on a skull and applying a vibrating tuning-fork to it. From these experiments he concluded that vibrations were restricted by surrounding less elastic parts, like the vibrations of a drumhead. The petrous bone, however, he concluded, owing to its anatomical structure and relations, vibrates like a rod held by one end in a vice. He refers to Weber's

[1] Baum, W. Beiträge zur Lehre von den indirecten Schädelfracturen. Langenbeck's Archiv f. klin. Chir., Bd. xix. S. 381–399, 1876.

wave theory, and concludes that blows upon the skull do not furnish the conditions of repeated impulses which produce a high vibratory action. By compression of a few skulls in a suitable frame he got results which correspond to those of later German experimenters, who adopted the "bursting" theory.

In 1878, P. Bruns attributed indirect fractures to the elasticity of the skull, as described by v. Bruns in 1854, and explained the frequent occurrence of these fractures in the base of the skull by the fact that this is the weakest and most fragile part of it.[1]

In the same year, Perrin made some very instructive experiments, in which he attempted to imitate the usual conditions of the skull by protecting it with an elastic cushion of cotton wadding or of caoutchouc and then throwing it upon a stone pavement, or by putting the cushion on the pavement itself. He always got direct fractures by blows, and by precipitating the skull he usually got indirect fractures. He showed typical specimens to the Société de Chirurgie, in all of which the fractures were indirect and meridional, and in some interesting cases the vitreous alone was involved in certain places. He found that blows on the vertex or occiput usually cause fractures by contrecoup (which he defines as fractures occurring at a place other than that struck), direct fractures being rare exceptions; while blows on the forehead, the parietals, or the temporal bones, rarely cause any but direct fractures.[2]

In 1880, Bergmann published another valuable con-

[1] Bruns, P. Die allgemeine Lehre von den Knochenbrüchen. Deutsche Chirurgie, Lief. 27, 1 Hälfte.

[2] Perrin, Maurice. Les fractures du crâne par contre-coup. Bull. et. mem. de la Soc. de Chir., 1878, pp. 128-136.

tribution to this subject.[1] He compared the elastic conditions of the skull to those of the thorax, which, when compressed or struck in one part, breaks in another. He also compares the mechanism of fractures of the skull to the bursting of the eyeball under pressure, referring to Arlt's demonstration[2], that when compressed the eyeball bursts in the equator. Fractures from severe blows, he believes, follow this law; those dependent upon moderate force follow the law laid down by Félizet. He analyzes the statistics of Prescott Hewett and of Schwartz, already referred to, and finds that a majority of their cases did not follow Aran's law. He calls attention to the importance of duly estimating the variations in amount, direction, and velocity of the force applied to the skull, and the support afforded by strengthening bones of the skull and face. He cites Baum (whom I have cited) as refuting the vibration theory, and seems to have the idea that indirect fractures of the skull are to be attributed to a disruptive force, and not to a propagation of vibrations.

In 1880, Messerer[3] conducted a series of experiments which demonstrated more accurately than had ever been done before the elasticity of the cranium, and which made clear the fact that when the skull is compressed in one axis, the circumference at right angles to it is enlarged.

In 1881, Nicolai Hermann published an inaugural dissertation on fractures of the base of the skull, in

[1] Bergmann. Die Lehre von den Kopfverletzungen. Deutsche Chirurgie (Billroth and Luecke), Lief. 30. Stuttgart, 1880.

[2] Arlt. Die Verletzungen des Auges mit besonderer Rucksicht auf deren gerichtsärtzliche Würdigung. 1875. S. ii.

[3] Messerer. Ueber Elasticität und Festigkeit der menschlichen Knochen Stuttgart, 1880. Cited by V. Wahl, *q. v.*

which, besides an analysis of 75 cases gathered from various sources, he gives an account of 17 experiments in which he compressed the skull in longitudinal, transverse, and diagonal directions, until it broke. His experiments always resulted in a fracture parallel to the direction of the compressing force. He believes that fracture always begins at one of the points compressed and travels away from it. By analyzing the cases he had collected he concluded that they supported his deductions from experiments.[1] It seems to me that some of his figures show plainly that the fissures must have begun at the middle and travelled both ways.

In the same year Julius Schranz published an account of 34 experiments upon skulls, some (13) from which the vault had been removed, but the most of them entire, in which he endeavored to imitate the conditions in which fractures are caused by different sorts of violence. The experiments were interesting and instructive. The great majority of the fractures he produced were splitting fractures, and *not* immediately connected with the point to which the violence was applied. In a considerable number isolated pieces of the vitreous table were split off, or the dorsum ephippii. These (and other indirect fractures), he says, can only be explained by the vibration theory. A curious result of his experiments was that the brain was injured only once, and the dura mater separated from the bone only three times![2]

But it remained for von Wahl,[3] in 1883, to utilize

[1] Hermann, Nicolai. Experimentelle und casuistische Studien über Fracturen der Schädelbasis. Inaug. Diss. 8vo. pp. 67, 23 plates. Dorpat, 1881.

[2] Schranz, Julius. Untersuchungen über das Entstehen von Schädelbrüchen. Mediz. Jahrbücher (Wien), 1881, pp. 291–314, with 5 lithographic plates.

[3] Wahl, Ed. von. Ueber Fracturen der Schädelbasis. Volkmann's Sammlung klin. Vorträge, No. 228. 8vo. pp. 26. Leipzig, 1883.

the material which had been recently gathering, and to formulate unequivocally the theory that fractures of the skull may be divided into: 1, crushing fractures, in which the line of fracture runs at right angles to the axis of the force applied; and, 2, bursting fractures, in which the lines of fracture are parallel to the axis of the force which gives rise to them, and which begin, in the words of Messerer, at some point in a line which, like the equator, in relation to the poles of the earth, circumscribes the hollow figure in a plane equally distant from both points of compression. He claims that, in compression of the skull—which may be gradual, as in a vice, or sudden, as when effected by a fall or blow—its elasticity, *in toto*, comes into play, and while its diameter which is parallel to the force applied is shortened, its diameters which lie at right angles to this are lengthened. The result is, that indirect fractures run in lines which we may describe as meridional. His conclusions rest not only on theoretical considerations and a review of the testimony and opinions of others, but also upon his own clinical experience, and a series of ingenious experiments, which are fully described and beautifully illustrated in his monograph, a careful study of which may, without injustice, be said to be indispensable to one who would form a correct opinion upon the subject of which it treats.

In 1884, Dr. Nancrede, of this city, published an elaborate and valuable paper on "Injuries of the Head,"[1] in which he adopts the vibration theory, and supports his opinions with some interesting anatomical and

[1] Nancrede, Charles B. Injuries of the Head. International Encyclopædia of Surgery. Edited by John Ashhurst, Jr., M.D., etc. New York, 1884. Vol. v. pp. 1–109.

physical considerations. He regards the base as a stronger part of the cranium than the vault (pp. 30 and 32), speaks of the brain as lying on a water-bed (p. 31), and describes vibrations of the bones as travelling by the nearest "anatomical" route to the base, there to be discharged, like electricity, at certain points—the ends of the petrous bones and the clinoid processes—into tissues which are non-conductors of vibration. He also calls attention to the important influence which the position of the head has upon the direction in which a force applied to the condyles will be conducted. In a very interesting case, which he cites from his own experience, there was a separation of the masto-occipital suture, which he says was forced apart "as if from within."

In 1884, Messerer published a second and most admirable paper on fractures of the cranium,[1] in which he gives a description and analysis of eighty-two experiments on fresh skulls, sometimes on entire cadavers. His experiments included accurately regulated blows upon skulls, detached or resting on the spinal column, and falls of the skull upon a hard base. These experiments completely refute the laws of Aran as well as those of Félizet, and seem to show conclusively that indirect fractures of the skull are dependent upon separation of the meridians caused by the depression produced at the point of contact by a blow.

Finally, our sketch of the history of this subject is brought down to the present year by a reference to the

[1] Messerer, Otto. Experimentelle Untersuchungen über Schädelbrüche. 8vo. pp. 36, with 8 plates. München, 1884.

interesting and beautifully illustrated paper of Greder, published in the early part of 1885.[1]

The experiments of Greder were made upon skulls connected with the trunk and still covered with their integuments and containing the brain, by blows with a weight of 6400–7650 grammes (about 12–15 pounds). After a large number of these experiments, some of which were most ingeniously varied, Greder comes to the conclusion that solutions of continuity of the base of the skull are to be regarded as the result of bursting; that the direction of all fissures is parallel to that of the force causing them; that the extension of a fissure is dependent upon so many intercurrent conditions peculiar to each skull, that it can at most be only suspected from the intensity of the violence (p. 509). As to the point at which the burst begins, he holds that it is not always in the equatorial line, nor always at the point of impact, but at that point where resistance to the disruptive force is least. This will happen, he thinks, in the majority of cases, in the equator or in its neighborhood (p. 506).

The Elastic Properties of the Skull.

From the preceding sketch, it will be seen that those who have thoroughly investigated the mechanism of indirect fractures of the skull have been led to conclusions which establish a great importance for its elastic properties. These properties, as we have seen, were first clearly enunciated about the middle of the last century by Saucerotte, Sabouraut, and Chopart, in their

[1] Greder, Wilhelm. Experimentelle Untersuchungen über Schädelbasisbrüche. Deutsche Zeitschrift für Chirurgie, Bd. xxi., 5 und 6 Hefte, 9 März, 1885, S. 491–510, tafeln vii.–xiv.

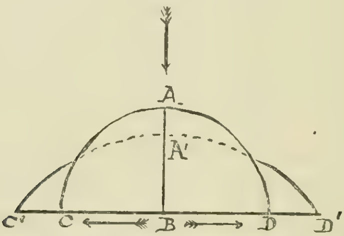
essays for the prize of the Academy of Surgery of Paris. In the beginning of this century, Sir Charles Bell illustrated them by an ingenious physical experiment. In 1854, von Bruns demonstrated them by a series of experiments which only required multiplication and variation to confirm what an important part the elasticity of the skull plays in the production of indirect fractures. Since then, almost innumerable experiments and studies of specimens of fractures of the skull have coöperated to establish the conviction that, with certain exceptions, the mechanism of indirect fractures of the skull may reasonably be explained according to what the Germans call the "bursting" theory—that is, to the conversion of direct depressing force into an indirect disruptive force, brought about by a shortening of the axis parallel to the direction of the force and a complementary lengthening of the axes at right angles to the former. A mass of evidence in support of this view is now accessible to any one who cares to examine it. It would not be possible to lay it all before you; so I shall select what I think most suitable to this occasion, and ask your attention to certain results obtained by Messerer, whose experiments in this connection have been very numerous and may be regarded as typical. In doing this, I intend no reflection upon the admirable work of Aran, von Bruns, Félizet, Baum, Perrin, Hermann, Schranz, von Wahl, and Greder, which must be studied to be sufficiently appreciated.

The experiments of Messerer can be analyzed so as to throw a great deal of light upon the physical properties of the skull, revealing the parts in which it is strongest, and those in which it is weakest.

But, in order to understand their significance, we

must have a clear idea of the mechanism by which, in a hollow case, the shape of which has a general resemblance to that of a spheroid or ellipsoid, a compressing force is converted into a disruptive force. It will be readily seen that when an elastic spheroid is compressed in any diameter, all the diameters lying at right angles to this—that is to say, in planes parallel to that of the equator—must be elongated. The experiment of pressing on the convex side of a bow, the ends of which rest on the ground, will illustrate what takes place in every chord of every arc that is depressed. (See Fig. 1.)

Fig. 1.

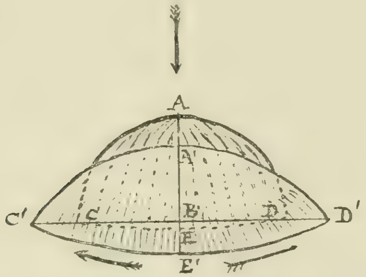


When a force is applied to A so as to shorten the axis AB to A′B, the axis CD will be lengthened to C′D′, and the compressing force in the line AB is converted into a disruptive force acting at right angles to this—that is, in the line CD.

It is equally clear that the elongation of the diameters lying in any plane will elongate the circumference of this plane, because a circumference of longer radius is longer than a circumference of shorter radius. As a result of this elongation of the circumferences parallel to the equator in any spheroid or ellipsoid, the meridians passing from pole to pole will be separated in a direct ratio to the extension of the different diameters and circumferences. This operation is illustrated whenever an

umbrella is raised. (See Fig. 2.) As a consequence of the separation of the meridians in a hollow sphere or ellipse, particles which before occupied a certain space between two meridians, will be compelled to separate in order to occupy the increased space between the meridians. The consequence will be that, in every case such as we have supposed, there will be a struggle between the cohesion of the particles and the disruptive force due to the separation of the meridians, and whenever the disruptive

FIG. 2.



When in a spheroid a force is applied to A so as to shorten the axis AB to A′B, all the axes at right angles to it are lengthened, and the circumference of any plane in which they lie will be lengthened, as CED to C′E′D′, and the meridians passing round the spheroid will be separated. Thus the direct depressing force is converted into a disruptive force acting at right angles to it.

force overpowers the cohesion of the particles lying along and between two of the meridians, a solution of continuity will take place. The disruptive force will be at a maximum at the equator, but the same conditions of disruptive force opposed to cohesion will be found in different proportions in every line parallel to the equator. As a result, in a perfect sphere with homogeneous walls such a solution of continuity would naturally begin at the geometrical equator and extend equally and simultaneously in opposite directions in a

meridional line toward the poles. But in a spheroid or ellipsoid not regular in shape, and with walls varying in strength in different parts, the result would be modified by these variations. This is exactly what we find in the case of the skull, which, while bearing a certain resemblance to an ellipsoid, does so in only a modified way, and can only be expected to exemplify the law just stated subject to modifications due to its own peculiarities.

With this in mind, let us see what the experiments of Messerer show. Messerer found that the skull burst in the base under an average pressure of 650 kilogrammes applied in a longitudinal direction, and under an average pressure of 520 kilogrammes applied in a transverse direction. From this we may draw two very important deductions. First, because the burst took place in the base:

Corollary I. The cohesive power of the base of the skull is less than that of the vault.

Second, because the skull burst under a pressure of 529 kilogrammes applied transversely, and under a pressure of 650 kilogrammes applied longitudinally:

Corollary II. Cohesion is less in the coronal zone of the skull than in the sagittal zone—that is to say, the skull is less able to resist a disruptive force due to blows upon the vertex or sides in the coronal zone than it is to resist a similar force caused by blows applied to the forehead or occiput. This is not only a fair inference from the experiments of Messerer, but it also accords with clinical observation.

Messerer found again that compression of the skull in a transverse direction, with a force of 520 kilogrammes, diminished the transverse diameter 4.4 millimetres; and

that compression in a longitudinal direction, with a force of 650 kilogrammes, diminished the longitudinal diameter only 2.7 millimetres. From this we may deduce

Corollary III. The skull is more compressible in the transverse diameter than in the longitudinal diameter. (This fact tends to strengthen the deduction of Corollary II.)

Messerer found, also, that compression of the skull in the transverse diameter, with a force of 520 kilogrammes, increased the longitudinal diameter 0.4 millimetre, and the perpendicular diameter 0.6 millimetre. From this we may deduce

Corollary IV. The skull is more extensible in the perpendicular diameter than in the longitudinal diameter of the sagittal zone. The natural inference from this corollary is that, under the influence of a force applied to the sides of the head a fracture which would naturally begin in some point in the sagittal zone, will begin in that part of it which is intercepted by the perpendicular diameter more readily than in that part of it which is intercepted by the longitudinal diameter—that is to say, at the base or vertex rather than at the forehead or occiput. We have already seen (Corollary I.) that the cohesion of the skull is less at the base than in the vault, therefore: of the two points just indicated, the base is that of election. Here, again, clinical observation supports our deduction.

Messerer found, further, that compression of the skull in the longitudinal diameter with a force of 650 kilogrammes increased the transverse diameter 0.6 millimetre, and the perpendicular diameter 0.1 millimetre. From this we may deduce

Corollary V. The skull is more extensible in the transverse diameter than in the perpendicular diameter of the

coronal zone. (This is the converse of Corollary III.) The natural inference from this corollary is that, under the influence of force applied to the forehead or occiput, a fracture which would naturally begin at some point in the coronal zone will begin in that part of it which is intercepted by the transverse diameter more readily than in that part of it which is intercepted by the perpendicular diameter—that is, at the sides rather than at the base or vertex. We have already seen (Corollary I.) that cohesion is less in the base than in the vault; therefore, a fracture which, for the reasons just stated, would naturally begin at the side of the skull would be more likely to occur near the base than near the vertex. In estimating this probability it may also be borne in mind that the horizontal equator of the skull lies nearer to the base than to the vertex, the curvature of the former being much less than that of the latter: so that the point of election for the fractures we are now discussing would naturally lie low down on the side of the skull. This deduction also is supported by clinical observation.

Messerer found, again, that compression of the skull in the transverse diameter, with a force of 520 kilogrammes, increased the longitudinal diameter 0.4 millimetre and the perpendicular diameter 0.6 millimetre; and that compression of the skull in the longitudinal diameter with a force of 650 kilogrammes, increased the transverse diameter 0.6 millimetre and the perpendicular diameter 0.1 millimetre. From this we may deduce

Corollary VI. The skull is more extensible along the sagittal equator than it is along the coronal equator. We have already seen (Corollary III.) that the skull is more compressible in the transverse diameter than it is in the longitudinal diameter. It is, therefore, seen that the equa-

tor along which the skull is more extensible (the sagittal) is that one the plane of which is cut at right angles by the diameter in which it is more compressible (the transverse), and *vice versa;* which furnishes in the skull a demonstration not only of the elongation of the equator which is produced by depression of the poles of a spheroid, but also of the direct ratio of this elongation to the depression. Furthermore, it appears from the observation of Messerer just cited, that a smaller force applied to the skull in a transverse direction will produce a greater elongation of the corresponding equator than will be produced by the application of a decidedly greater force in a longitudinal direction. From this we may draw the deduction that force applied to the side of the head is more likely to produce a fracture crossing the sagittal equator than force applied to the forehead or occiput is to cause a fracture crossing the coronal equator. This deduction is in accord with the results of Messerer's experiments, in which the skull burst under a pressure of 520 kilogrammes applied in a transverse direction, and only under a force of 650 kilogrammes applied in a longitudinal direction. It is also in accord with clinical observation.

We have now analyzed Messerer's experiments so as to ascertain the points of election for disruptive fractures which may occur in (that is, across) two of the principal equators of the skull, the sagittal and the coronal. It is not possible to discover in the same way the points of election in the horizontal equator of the skull; for Messerer found that compression of the skull in the vertical direction, between the vertex and the spinal column, led to a direct driving in of the base under so small a pressure as 270 kilogrammes.

The results of our analysis of these experiments may be summarized as follows: 1. Bursting fractures are more likely to occur at the base of the skull than in the vault. 2. Force applied to the side of the head may be expected to produce a bursting fracture crossing the sagittal equator. Such a fracture is more likely to occur in the base than at the vertex. 3. Force applied to the forehead or occiput may be expected to produce a bursting fracture crossing the coronal equator. Such a fracture is likely to occur in the temporo-parietal region, and nearer to the base than to the vertex. 4. Force applied to the vertex or base of the skull may be expected to produce a fracture at the base.[1]

Anatomical Peculiarities of the Skull.

Having now briefly considered the elastic properties of the skull, let us next study its anatomical peculiarities, which, as I have already pointed out, may be expected to modify the results which might be expected if the shape of the skull were regular and its thickness uniform. These peculiarities must be given due consideration if we would avoid the error of applying too rigorously the bursting theory; at the same time, they must not be overestimated. This was the error of Félizet, who was right in attaching great importance to the architectonic conditions of the skull, but who was mistaken in supposing that his buttresses and centre of resistance would stand firm against all or most fractures.

[1] It may be worth while to call attention to the fact that there is no essential difference between a blow applied directly upon the vault or side of the skull and one transmitted through the spinal column by a fall upon the feet or buttocks.

So Aran, before him, was right in supposing that fractures starting in one fossa of the skull would often be limited to that fossa; but he erred in supposing this could be stated as a general law. Both of these theories depended upon what we may call the accidents of fractures of the skull; the essential, I believe—and the future may show that this view may be applied too rigorously—is to be found in the elastic properties of the skull, as revealed not only by the experiments of von Bruns and of his German followers, but also by those of Aran and of Félizet themselves, and by the investigations of others before their time, to whom allusion has already been made in the historical sketch at the beginning of this paper.

The thinnest portions of the various bones of the skull are as follows:

In the frontal bone. The walls of the frontal sinuses; the orbital plates; the temporal portions; and sometimes on both sides of the crest for the attachment of the falx cerebri.

In the ethmoid bone. The horizontal plate. (The other parts of the bone may be regarded as outside of the true cranial case.)

In the sphenoid bone. The orbital portion of the greater wings, and the walls of the sphenoidal sinuses. The basilar portion of this bone has a deceptive appearance of strength, on account of its thickness; but on section it will be seen to be of such open cancellated structure that it is not surprising to find that it is very often fractured.

In the parietal bones. The lower border, just above the parieto-squamosal suture, and both of the inferior angles.

In the occipital bone. The floor of the cerebellar fossæ; and, to a less extent, the floor of the posterior cerebral fossæ.

In the temporal bones. I take these bones last because they present some extremely interesting anatomical peculiarities, the importance of which I have not found dwelt upon as I think it ought to be. The thinness of the squamous plates is too familiar to need more than mention. But the temporal bone is also often very thin over the cotyloid depression for the head of the inferior maxilla. The mastoid portion is also sometimes occupied by such large open spaces as materially to weaken its walls. The petrous portion does not, on section, present everywhere that solid and rocky appearance to which it owes its name, and which it only partly deserves. The roof of the external auditory meatus is sometimes very thin, in a line parallel to and anterior to the superior ridge of the bone. The floor of this canal is also comparatively thin. The plate of vitreous which covers the anterior inclined plane of the bone is often a mere shell over the promontory above the superior semicircular canal, and further back, in front of the groove for the superior petrosal sinus, it overlies a mass of honeycombed cancellated tissue which communicates with the large cells of the mastoid process. These facts explain the many fractures which are found to run parallel to the crest of this bone, and which have often been wondered at. In like manner, the curious course which a fracture sometimes takes at right angles to the main axis of the petrous bone may be understood when we study the channels and excavations belonging to the middle and internal ear, the

internal auditory meatus, the carotid canal, the groove for the inferior petrosal sinus, and the jugular foramen.

These are some of the weak points of the bones of the skull. Others might be mentioned; as, for example, the antero-lateral rim of the foramen magnum, which, in some cases, is very porous, and far from so strong as its thickness would indicate. It will be noticed that all of these thin parts of the skull lie in or near the base, and clinical observation shows that they are very frequently the seat of fracture.

There are a few other anatomical peculiarities of the skull to which I would like to call your attention before I leave a part of our subject which might well occupy much more time than we can spare it. First, in regard to the posterior clinoid processes. It was long a matter entirely unexplained that violence done to the skull was followed by no fracture, except one breaking off the posterior clinoid processes of the sphenoid bone. But the explanation is easy enough if we accept the "bursting theory," and recognize the fact that the tentorium cerebelli is attached in front to these processes, and that when the long diameter of the skull is increased the tense tentorium holds back with an unyielding strain and tears these processes from their attachments to the rest of the bone. In this we have both a full explanation of the mechanism of these fractures and a beautiful confirmation of the "bursting theory" of indirect fractures of the skull.

Again, let me call your attention to the peculiar conditions of the basi-sphenoid and petrous bones. These bones are never, I believe, united by bony union or close-fitting dentations, like the other bones of the skull. Their adjacent surfaces are simply applied

against each other and united by a membranous or ligamentous band. The plane in which these bones articulate is such that the basi-sphenoid is placed like a wedge between the apices of the two petrous bones. As a consequence, any force, like a blow on the vertex, driving the cranium down upon the spinal column, or a fall upon the buttocks or feet, driving the spinal column up against the cranium, will have the effect of forcing the wedge-shaped basi-sphenoid upward and forward between the two rigid petrous bones, with the frequent result of knocking off one or both of the apices of the latter. This form of fracture is another which has excited much surprise and some fanciful explanations; but, in the light of what has just been stated, it seems to me to be quite comprehensible.

Finally, I have been struck by the fact that certain peculiar fractures of the base of the skull seem to have been due to support of a segment of the occipital bone furnished by the inclination upward and outward of the articular surfaces of the atlas, which embrace the condyles of the occipital bone and restrain it from rupture to such an extent that a fissure will pass on both sides of the protected region rather than through it. This form of fracture I illustrate by a beautiful specimen taken from the Mütter Museum of this College.

Architectonic Peculiarities of the Skull.

I cannot now do more than allude to the general oval shape of the skull, its various curves, arches, and buttresses, the comparatively even shape and homogeneous character of the vault, and the irregular shape and varying thickness and thinness of the base, with its promi-

nences and depressions, its channels and its so-called sinuses. These are too complex to be detailed here, and too familiar to all to make this necessary. But I would call your attention to what Félizet has laid so much stress upon, the reinforcement of certain regions, as at the junction of the greater wings of the sphenoid bone with the frontal and temporal bones, and of the petrous portion of the temporal bones with the parietals; at the ridge for the attachment of the longitudinal and lateral sinuses within the skull and at those for the attachment of muscles at the back of the head, which vary in importance in different individuals. In addition, we may well bear in mind the comparative roundness of the vault and flatness of the base, and the wedge-like insertion of the lower borders of the parietal bones between the bevelled upper edges of the squamous bones, and the projection of the ends of the petrous bones against the basi-sphenoid, of which I have already spoken in detail. For most of the points one may consult the works on general and special anatomy, or, better still, the skull itself. They all must be considered in studying variations from the result to be expected from the application of any given theory in regard to the mechanism of fractures of the skull, although, as I have said, they need not be expected to controvert any law of general applicability.

Influence of the Soft Parts and Contents of the Skull.

This is another portion of the study of fractures of the skull to which more time ought to be devoted than we can spare now. In a general way, it may be said

that the external and internal coverings of the skull—the dura mater and the pericranium, with the muscles, fasciæ, and skin—tend not only to deaden vibrations to such an extent that it is hard to know how to understand many expressions of those who have accepted what is known as the "vibration theory," but also to limit to a moderate extent the changes in shape which the skull may undergo, according to the "bursting theory." The action of the muscles must also be considered of importance, as exemplified in such involuntary and spasmodic contractions as prove sufficient of themselves to break other bones, and which may have more effect here than has yet been suspected. Let us imagine, for example, what might be the effect of a spasmodic contraction of the erector mass at the back of the head when a sudden blow comes upon the anterior part of the vertex, tending to flex the head. I wish it were possible to go further into this inviting field of investigation.

As to the contents of the skull, we have seen that Fabricius ab Aquapendente attributed fractures by counterstroke to the effort of the air, which he supposed the skull to contain, to escape from its cavity. We know better than this; but it does not seem needless to call attention to the fact that the skull is filled with a much more incompressible material than air. In fact, the brain and its membranes, with the surrounding fluid, completely fill the skull with one of the most incompressible materials of which we have any knowledge. Under the influence of a blow, this material tends to increase the disruptive force, according to the well-known laws of hydrostatics. As a full cask may be burst by a blow, so there can be no doubt the skull may

likewise be burst. The statements often made, which affirm or imply that the skull and its contents can be compared to a water-bed, are utterly erroneous. The fluid contained in the cranium, in a state of health, is far too little to justify any such comparison. Besides which, it is contained in a firm, unyielding case; while it is indispensable to a water-bed that its walls shall be soft and yielding. It has also been intimated—and so able a man as Mr. Hilton thought he had demonstrated—that displacement of the cerebro-spinal fluid could be regarded as a protective against injury when the skull was struck. Mr. Hilton's experiment was not free from an important source of error, and one who studies carefully the anatomical peculiarities of the cerebro-spinal axis must see, I think, that there is only one way for compensatory diminution in the contents of the skull to take place, namely, by displacement of its contained blood through its natural channels. This displacement can and does take place when time is afforded for the process, as in the case of a growing tumor; but the rapidity with which a blow tends to diminish the capacity of the cranium far outstrips the speed with which the blood can escape; so that, as a matter of fact, the skull and its contents may be regarded as the very opposite of a water-bed, and more like a cask filled with fluid which may burst under a sudden increase of the pressure upon the inside of its walls. This fact is established not only by a correct knowledge of the physical conditions of the cerebro-spinal axis, but also by experiments and observations which I cannot stop to mention now.

Study of Cases.

A great difficulty confronts at the outset one who tries to discover in how far the study of actual fractures of the skull will bear out any theory in regard to their mechanism which seems plausible. The difficulty depends upon the difference between the known factors in an experiment and the unknown factors in an accident. It is often impossible, in the case of a fracture of the skull, to ascertain exactly the amount and direction of the force, the point to which it was applied, and the conditions of resistance or evasion of the whole cranial box. This consideration should lead to a modest and reserved fitting of what seem to be facts to any theory. Nevertheless, nothing can be learned by standing still; and we may, if not overconfident, make some advance in knowledge by testing a theory by an observation of what actually takes place. With this object in view, I have examined the published accounts of a very large number of cases of fracture of the skull, and I have collected more than a hundred (119) in which the details seem to me to be sufficient to warrant an approximative estimate as to the conditions under which the fracture occurred, and as to the results produced. I cannot go over the records of these cases in detail, but I now show you drawings of all of the fractures, each accompanied by a reference to the source from which I have obtained it, and such verbal explanations as will, I trust, enable you to form some opinion as to the amount of support they can be credited with affording to the "bursting theory" which I have already described and endeavored to explain. In presenting

these drawings to your attention, I must warn you against certain sources of error in them. The first depends upon the imperfection of the great majority—I might almost say of all—of the records from which they have been made up; the second depends upon the great difficulty of representing upon a flat surface what may be gathered from a verbal description of so complex and intricate a thing as a fracture of the skull.

In addition to the drawings representing each case of fracture by itself, I show you one in which I have transferred to a single diagram all the fracture lines falling within the base of the skull. I do this to indicate, if it be possible, where fractures most frequently occur, and also to make clear the fact that the parts which Félizet has assumed to have peculiar strength—the petrous bones and the basilar portions of the occipital—seem, on the contrary, to be the very seats of election for fractures of the base.

For convenience of comparison, I have arranged the drawings in classes according to the nature and direction of the violence which produced them. Wherever there seemed to be doubt as to these points I have indicated this fact by a query mark (?). The direction of the violence is indicated, wherever this could be done, by an arrow pointing to the point of impact.[1]

CLASS A^1. *Fractures caused by blows on the frontal region.* Of this class I have only two cases. One caused by a kick from a horse above the right eyebrow, causing a direct fracture here, and an indirect meridional and oblique fracture of the parietal bone of the same side. The other case presents a fracture caused by a blow

[1] The plates published at the end of this paper have been prepared from those presented to the College, so that a single diagram may exhibit a number of fractures caused by the same sort of force.

with the handle of a pitchfork, high up on the frontal region. The fracture is in the base, is meridional, and passes from the right orbital plate through the sphenoid bone and the basilar process of the occipital, leaping across the foramen magnum, from the back edge of which start two converging fissures which meet in the cerebellar fossa on the left side, and end above the groove for the lateral sinus.

CLASS A^2. *Fractures caused by falls upon the frontal region.* Of these I have thirteen cases. Twelve present meridional fissures; five of them being directly longitudinal, three of these passing directly through the whole basi-sphenoid bone. Seven are oblique; two of them bifurcated, and one passing through the whole basi-sphenoid bone; one passes from the point of impact to the lower part of the ridge for the straight sinus, after having passed across the middle of the petrous bone and encircled the foramen magnum. One shows a ring fracture, in addition to a meridional fracture from the frontal bone to the junction of the petrous with the basi-sphenoid. One is directly opposite to the point struck, is extensive and bifurcated.

CLASS B^1. *Fractures caused by blows on the occipital region.* Of these I have only two cases. One was caused by a spent ball striking the occiput to the left of the middle line, and shows an independent meridional fracture passing alongside of the internal ridge to near the foramen magnum, then crossing the petrous bone, dividing it transversely to its principal axis, and ending in the foramen spinosum. The other case is a specimen from one of Perrin's experiments, in which the skull was thrown on a stone pavement covered with a layer of India-rubber. There is a separation of the coronal suture, and a prolongation of this as a bifurcated fissure in the right squamous bone. This specimen shows a division at right angles to the meridian, which may be due to an unusually weak union in the coronal suture, or to some peculiarity of the experiment. I have included this specimen among my drawings, because it is diametrically opposed to the bursting theory. But I think an experiment which involves throwing a skull detached from the body, cannot be considered very reliable in comparison with those in which the skull is fixed.

CLASS B². *Fractures caused by falls, striking upon the occipital region.* Of these I have twenty-one cases. All show meridional fractures. Two of these pass directly through the crest for the lodgement of the straight sinus to the foramen magnum. One passes to the foramen magnum near this crest. Two pass through the whole of one side of the occipital bone, and one splits off a piece of the parietal, and separates the lambdoid suture in part. One divides the frontal bone into halves. Six pass round the foramen magnum on one side, three dividing the petrous bone longitudinally and one transversely. One shows a longitudinal fracture passing across the foramen magnum and dividing the basi-sphenoid bone, as well as a partial ring-fracture. In two the fissure passes meridionally round the side of the head. One shows an independent fissure on each side, in the parietal bones.

CLASS B³. *Fractures due to blows on both the forehead and occiput.* One such case I have found recorded by Herpin, in which there was a succession of bumps on each of these parts. The resulting fracture is a long fissure, passing longitudinally from the internal occipital protuberance to the foramen cæcum, passing round the foramen magnum, very close to it, and dividing the whole of the basi-sphenoid: a beautiful meridional fissure.

CLASS C¹. *Fractures caused by compression in a longitudinal direction: accidental.* Hewett records two cases of this sort. In the first, a man fell on the back of his head, and a piece of timber fell on his forehead. The fissure is independent, and partly meridional. It divides longitudinally the middle of the horizontal plate of the ethmoid bone and the middle of the body of the sphenoid bone, passing then to the right through the greater wing of this bone, and bifurcating before passing in two fissures upon the squamous plate of the temporal bone. In the second, the history is far from clear, but it seems that the fracture was caused by the compression of a cartwheel passing across the forehead. The fracture is meridional and exactly like several in the next class.

CLASS C². *Fractures caused by compression in a longitudinal direction: experimental.* Of these I have nine cases. Six show meridional fissures. One divides the skull from front to back over the vault. Three divide the skull to an equal extent through the

base, passing round the foramen magnum. In two there are two independent fissures. In one of these cases one of the two fissures is not meridional. Three of the cases show complicated fissures which may be due more to crushing than to bursting.

CLASS D¹. *Fractures from blows on the parietal region.* Of these I have only four cases. All show meridional fissures. Three pass straight down to the apex of the petrous bone, and one passes also up to the vertex.

CLASS D². *Fractures caused by falls, striking on the parietal region.* Of these I have fifteen cases, in all of which I think the fractures may be considered meridional. Four pass along the horizontal equator of the skull. Seven are transverse, three crossing the base from side to side, two going half way across, one involving only the ends of the petrous bone, and one passing across the vault at the back part of the parietal bones, and then passing forward to the base. Four are diagonal, two of them passing across the sella turcica.

CLASS E¹. *Fractures caused by compression in a transverse direction: accidental.* Of these I have five cases. In all, the fractures are transverse and meridional. Two pass across the basi-sphenoid bone and upward on both squamous bones. In one both parietal bones are split independently, the fissures passing down through the petrous bone. In one the coronal suture was separated, and a piece broken out of each parietal, beside an independent fracture of the occipital and petrous bones at the base on one side.

CLASS E². *Fractures caused by compression in a transverse direction: experimental.* Of these I have five cases. In all, the fissures are transverse and meridional. In four the basi-sphenoid is divided transversely, and in three the fissure involves also the temporal bones. In one case the coronal suture is disarticulated.

CLASS F. *Fractures caused by falls, striking on the temporo-frontal region.* Of these I have only two cases. In one the fracture is meridional; in the other it is equatorial, and seems to illustrate the shoving of the posterior half of the skull over the anterior half by the impact of the spinal column on the base in an oblique direction.

CLASS G. *Fractures caused by compression in a diagonal direction: experimental.* Of these I have five cases. In four the fissures are meridional, two being quite complicated. In one case there is a separation of the posterior inferior angle of the parietal bone, and the dorsum of the sella turcica is broken off.

CLASS H^1. *Fractures caused by blows on the vertex: accidental.* Of these I have six cases. In all, the fractures are meridional; in five they are transverse and in one longitudinal.

CLASS H^2. *Fractures caused by blows on the vertex: experimental.* Of these I have three cases. All of them show beautiful meridional fissures, one transverse and two longitudinal, one of the latter dividing the skull completely into halves.

CLASS H^3. *Fractures caused by falls upon the vertex: accidental.* Of these I have seventeen cases. In all, the fissures are meridional and can, I think, be attributed to a burst. In one the front half of the skull is divided longitudinally, and in one the posterior half is similarly divided. In one the fissure divides the skull into halves longitudinally; in two it divides it into halves transversely. In one the basilar artery was found caught in a transverse fissure of the basilar process of the occipital bone.

CLASS J^1. *Fractures caused by falls upon the condyles (transmitted from feet): accidental.* Of these I have only three cases. In two both posterior clinoid processes were broken off—which can be attributed to the lengthening of the antero-posterior diameter of the skull—and there was a transverse fracture of the apex of the petrous bone. In the third case there is only a longitudinal fissure of the horizontal plate of the ethmoid bone on one side.

CLASS J^2. *Fractures caused by blows on the condyles: experimental.* Of these I have four cases. In all of them the dorsum of the sella turcica has been torn off by the elongation of the antero-posterior diameter of the skull. In two cases this is the only fracture. In one case the whole base is also divided transversely, the fissure passing across the basilar process of the occipital bone. In one case there are also three independent fissures, all transverse.

Conclusion.

The analysis of these 119 cases shows that 111 present fissures which correspond to what might be expected from an application of the principles of the "bursting theory," and only 8 seem to contradict it. This result, which has surprised me by its apparent completeness, seems to establish this theory by the best test which we can apply to it, so that it appears to rest upon a very firm tripod of reasoning, experiment, and clinical observation.

I trust it will not be supposed that, in making so much of the bursting theory in this paper, I have overlooked the fact that there are fractures which cannot be accounted for by it. There are some fractures in which the force applied is so great, and acts in such a manner, that the skull is crushed so as to hide any evidence of the play of its elastic properties, the fracture being of a comminuted sort; and there are others in which, as I have noted in passing, one segment of the skull seems to be shoved over the other by forces of pressure and counter-pressure which require some study before their mode of operation can be understood. In this connection it is of importance to learn in any case the position which the skull has held in relation to the spinal column, or to any body capable of exerting counter-pressure. No less is it important not to overlook the counter-pressure which is caused by the simple *vis inertiæ* of the skull, and its contents.

But it would be impossible to speak of all the influences which may modify the strict application of any one theory in regard to fractures of the skull. I have laid before you all the evidence which I now can in re-

gard to this matter; and I must close with the expression of my own conviction that the supreme law governing the production of indirect fractures, is that which depends upon the fact that the skull is practically a hollow elastic case, approximately oval in shape, and which may be briefly formulated as follows: When a sufficient force is applied to any curvilinear part of the skull, if this part do not give way immediately, the axis of the skull lying in the same line as that of the applied force is shortened; all the axes lying in planes at right angles to this line are correspondingly lengthened, with a proportional lengthening of their circumferences, and separation of their meridians; so that the direct depressing force is converted into an indirect disruptive force acting at right angles to the direction of the former. The effect is to produce a fissure, or fissures, which will have a general meridional direction.

The application of this law is subject to certain modifications due to the anatomical and architectonic peculiarities of the skull, its coverings and contents, and to certain exceptions due to the amount and velocity of the force applied as well as to the coming into play of peculiar counter-forces.

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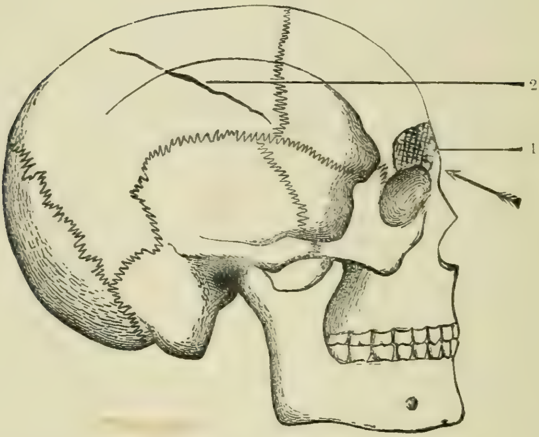
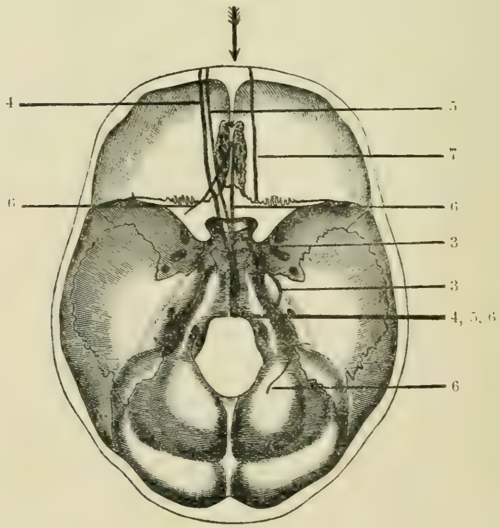
PLATE I.



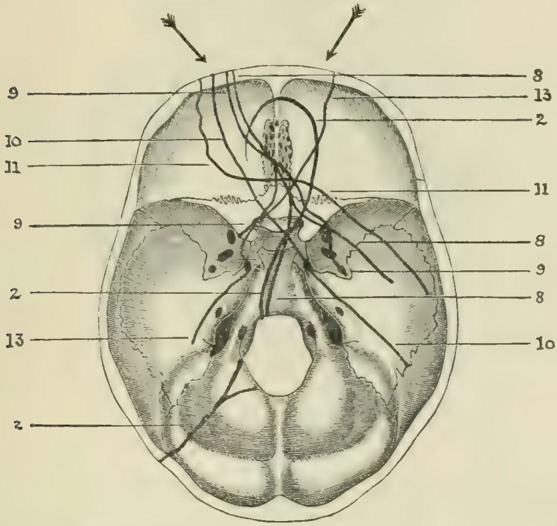
Independent, meridional fissure caused by kick by a horse; skull broken in above orbit.

PLATE II.



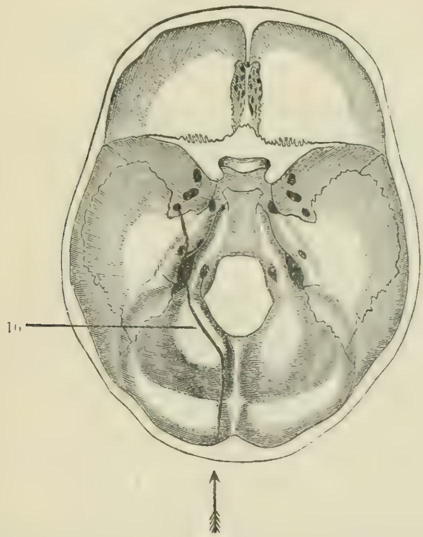
Fissures caused by force applied to forehead; by falls.

PLATE III.



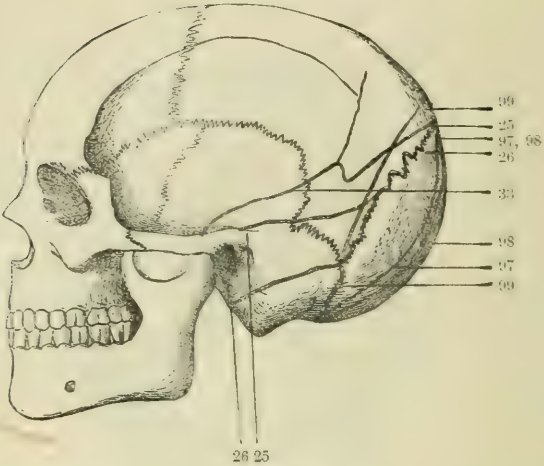
Fissures caused by force applied to forehead in an oblique direction; by falls. No. 11 continued up over frontal bone to beyond coronal suture on left side.

PLATE IV.



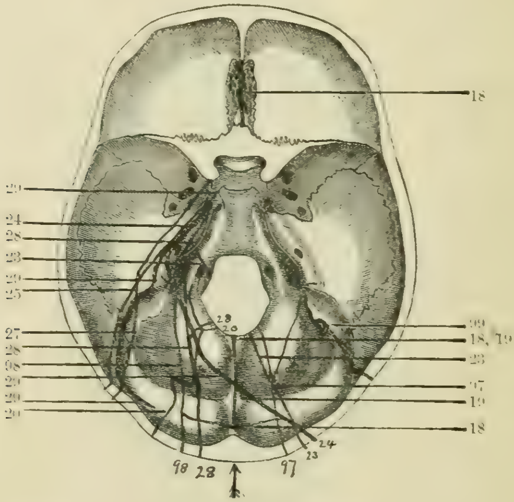
Fissure caused by force applied to occiput; blow by a spent ball.

PLATE V.



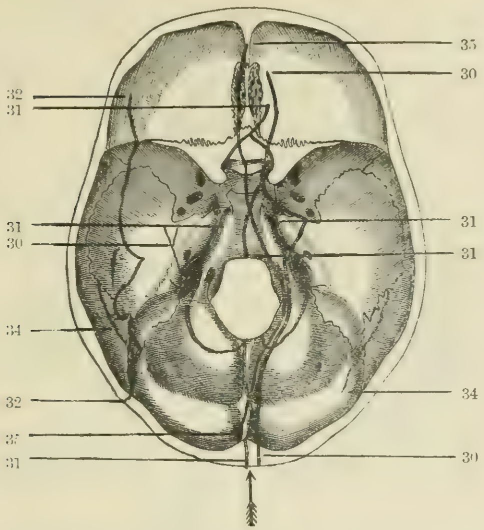
Fissures caused by force applied to occiput; by falls.
No. 25 continued to apex of petrous bone, through anterior inclined plane.
Nos. 33, 97, and 99, were on right side of skull. (See also Plate VI.)

PLATE VI.



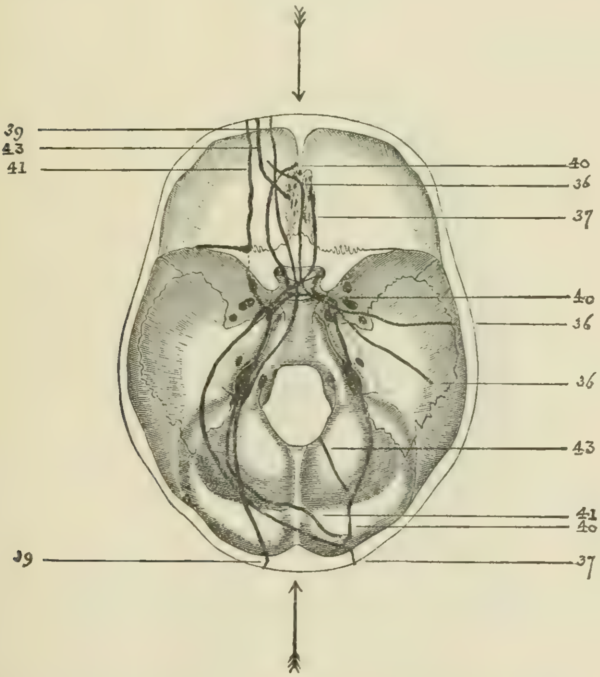
Fissures caused by force applied to the occiput; by falls.
No. 97 passed up on right to lambdoidal suture.
No. 98 passed up on left to lambdoidal suture.
No. 99 passed up in front of lambdoidal to near sagittal suture.
No. 25 passed round left side to root of zygoma. (See also Plate V.)

PLATE VII.



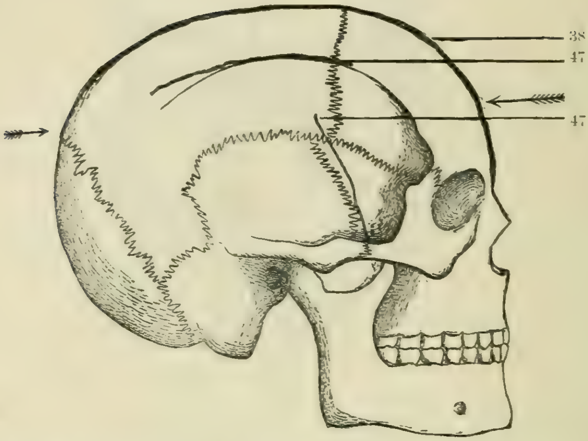
Fissures caused by force applied to occiput; by falls.
One fissure (No. 35) caused by force applied to occiput and forehead alternately.

PLATE VIII.



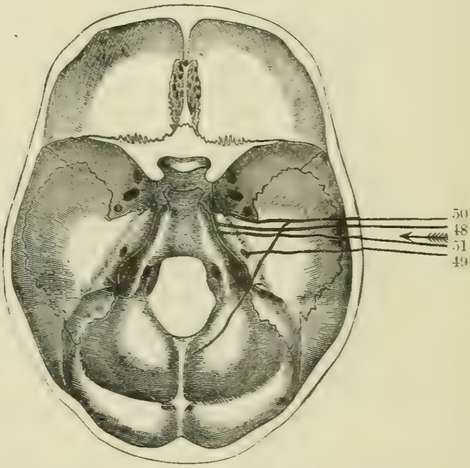
Fissures caused by compression in fronto-occipital diameter.
Nos. 36 and 37, accidental.
Nos. 39, 40, 41, and 43, experimental.
No. 37 passed up and forward on right parietal bone to near its middle.
No. 43, fissure in cerebellar fossa involved vitreous table alone.

PLATE IX.



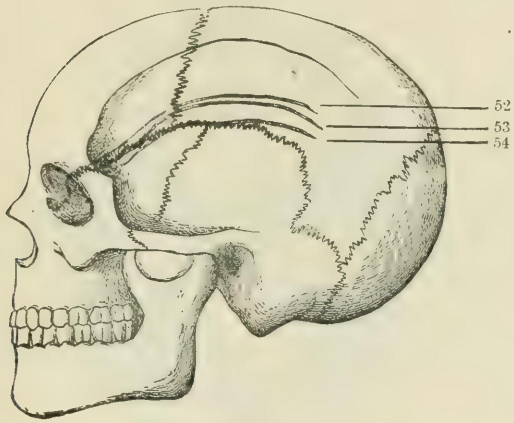
Fissures caused by compression in occipito-frontal diameter; experimental. No. 47 was on left side of skull.

PLATE X.



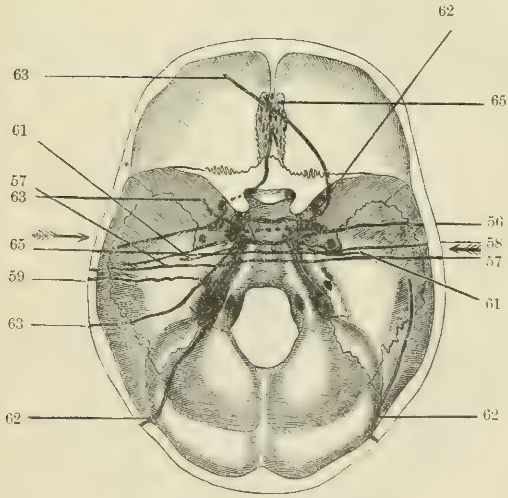
Fissures caused by blows on side of head.
No. 51 passed up to sagittal suture.

PLATE XI.



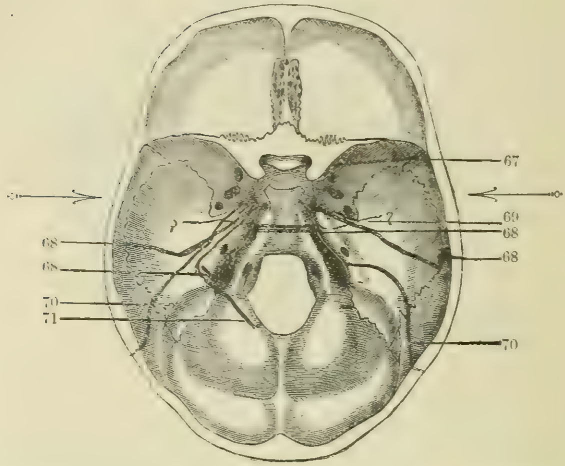
Fissures caused by falls, striking on parietal boss.

PLATE XII.



Fissures caused by falls, striking on side of head.
No. 62, fracture at back of skull passed over vault through both parietal bones, caused by fall on left parietal boss.

PLATE XIII.



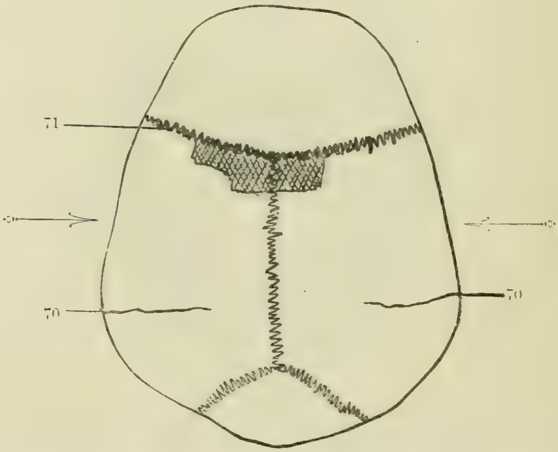
Fissures caused by compression in transverse diameter; accidental.

No. 69, extension of fissure into side of base imperfectly described.

No. 70, fissure on each side continued upward in coronal direction half way to sagittal suture. (See also Plate XIV.)

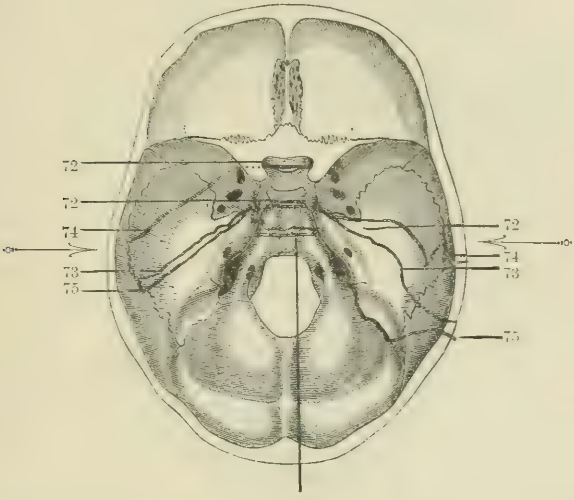
No. 71, principal fissure was in coronal suture; the fracture at base hard to distinguish, as patient survived forty years. (See also Plate XIV.)

PLATE XIV.



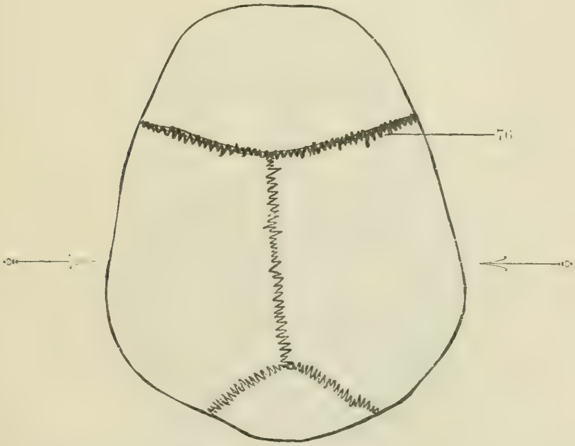
Fissures caused by compression in transverse diameter.
(See also Plate XIII.)

PLATE XV.



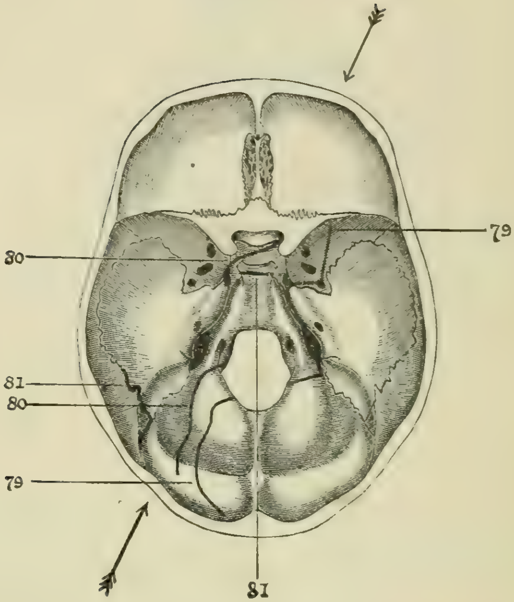
Fissures caused by compression in transverse diameter; experimental.

PLATE XVI.



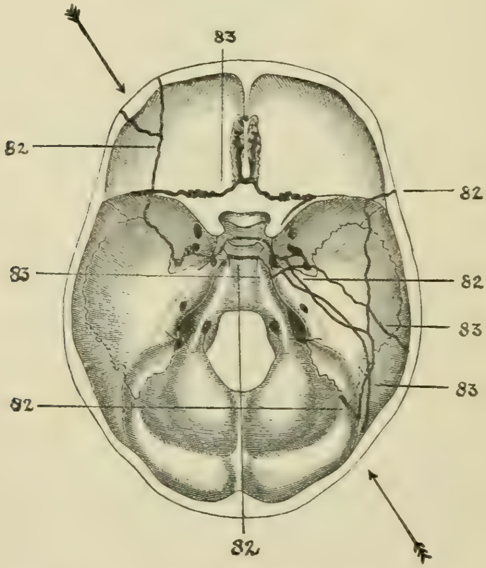
Fissure caused by compression in transverse diameter; experimental.
No. 76, separation of coronal suture.

PLATE XVII.



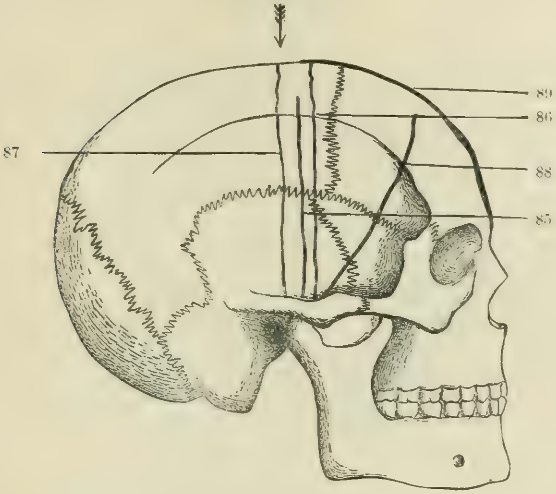
Fissures caused by compression in diagonal direction; experimenta .

PLATE XVIII.



Fissures caused by compression in diagonal diameter; experimental.

PLATE XIX.



Fissure caused by blows on top of head ; accidental.

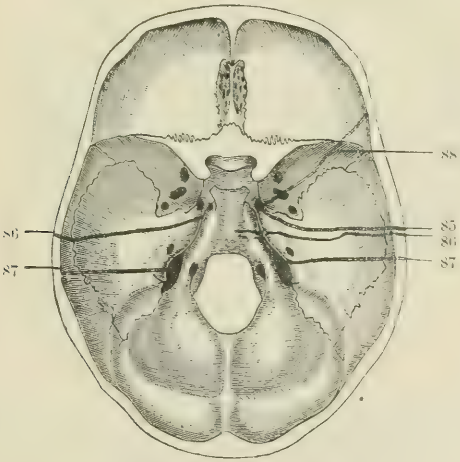
No. 85. See also Plate XX.

Nos. 86 and 87 passed across whole of vault and into base on both sides. (See also Plate XX.)

No. 88, fissure passed to carotid canal ; end of petrous bone also broken off. Child, four years old. Billet of wood fell considerable distance, struck on forehead. (See also Plate XX.)

No. 89, man struck on "crown of head" by a piece of iron falling a considable distance.

PLATE XX.



Fissures caused by blows on top of head ; accidental.

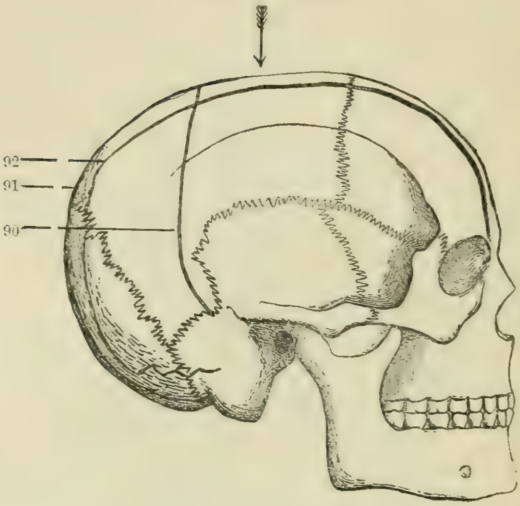
No. 85, fissure passed up to near sagittal suture. (See Plate XIX.)

No. 86, fissure completely bisected vault. (See Plate XIX.)

No. 87, vault crushed in and fissure connected with those in base. (See Plate XIX.)

No. 88, fissure extended up to centre of left frontal bone. (See Plate XIX.)

PLATE XXI.

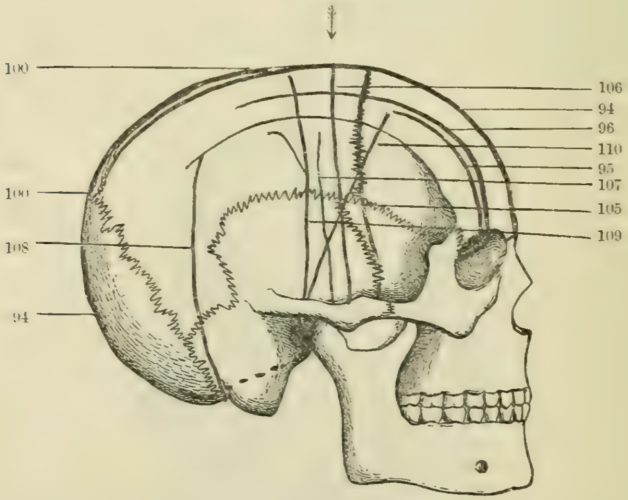


Fissures caused by blows on top of head; experimental.

No. 91 passed to foramen magnum to left of median line. Placed on right side here for convenience.

No. 92 divided base from front to back, splitting basi-sphenoid.

PLATE XXII.



Fissures caused by falls on top of head; accidental.

No. 94 divided skull into halves longitudinally. (See Plate XXIII.)

No. 95 passed to sella turcica. (See Plate XXIII.)

No. 96 passed to foramen magnum. (See Plate XXIII.)

No. 100 passed to foramen magnum. (See also Plate XXIII.)

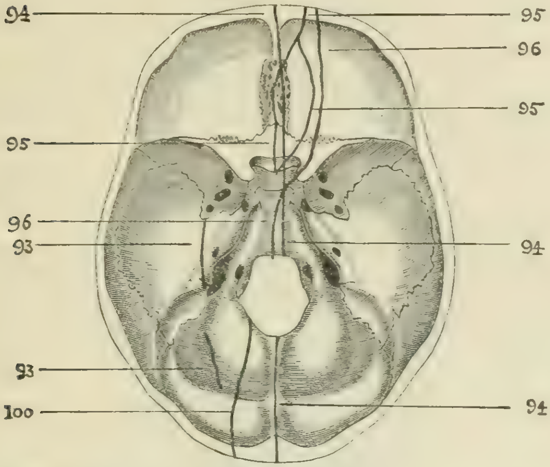
No. 106 divided skull into halves. (See Plate XXIV.)

Nos. 107 and 108 were on *left* side. (See Plate XXIV.)

No. 109, fissure passed to foramen magnum. (See Plate XXIV.)

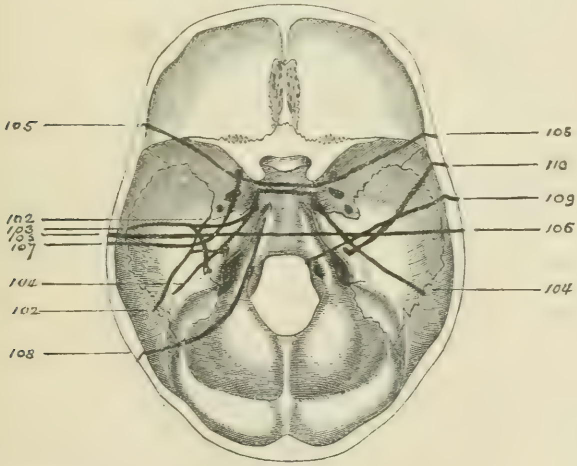
No. 110 fissure passed to jugular foramen. (See Plate XXIV.)

PLATE XXIII.



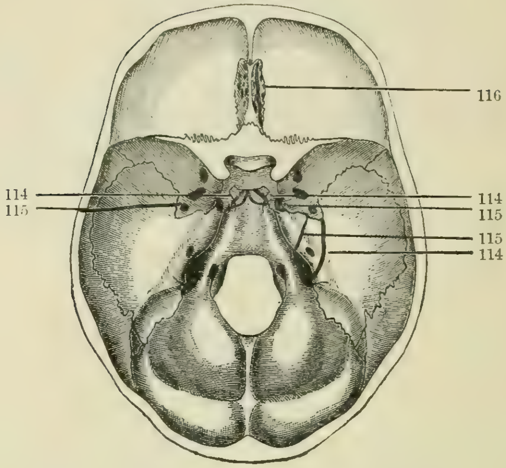
Fissures caused by falls on top of head; accidental.
No. 94, fissure divided skull into halves longitudinally. (See Plate XXII.)
Nos. 95, 96. See also Plate XXII.
No. 100 passed up to apex of lambdoidal suture and through sagittal to frontal bone. (See also Plate XXII.)

PLATE XXIV.



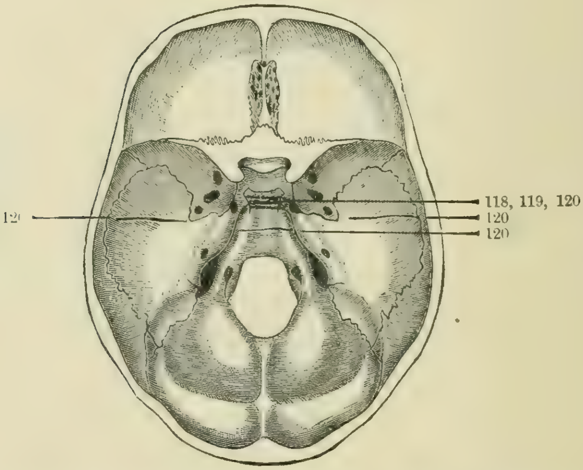
Fissures caused by falls on top of head; accidental.
Nos. 105 and 106 divided skull into halves. (See Plate XXII.)
No. 107 passed up on left side to middle of parietal bone. (See Plate XXII.)
Nos. 108, 109, 110 passed up to near vertex. (See Plate XXII.)

PLATE XXV.



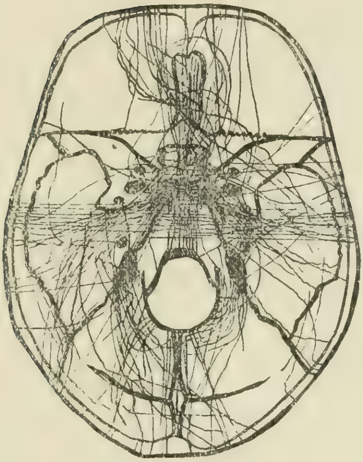
Fissures caused by falls on feet; accidental.

PLATE XXVI.



Fissures caused by blows on condyles; direct or transmitted; experimental.

PLATE XXVII.



Lines of fissures in 100 cases. It has been impossible to represent all that passed through the basi-sphenoid, either longitudinally or transversely, or those which passed along the anterior inclined plane of the petrous bone.

[After the reading of the preceding paper :—]

Dr. Agnew remarked: The paper just read is very interesting, and on a very interesting subject. I must say, however, that I cannot agree with Dr. Dulles as to the mode of fracture—the elasticity of the skull is too insignificant. I still hold to the old vibratory theory. I satisfied myself of the correctness of this by experimenting with ivory balls, suspended in such a manner as to touch different parts of the skull, and watching the effect on these balls when the cranium was struck. Fractures of the skull, in the vast majority of instances—when the force is concentrated on a very limited surface—occur at the point of impact. The force which gives rise to fractures at the base is usually a diffused one, and if applied at the vault of the skull travels to the base by the shortest route, the vibrations concentrating or focusing on certain fossæ in nearest relation with the seat of the applied violence.

Dr. Packard said: It seems to me that there are two points not mentioned by Dr. Dulles which can hardly be left out of the account in considering the subject of these fractures. One is the direction of the bony fibres—the intimate structure of the bones, as distinct from their mere shape. The other is the momentum of the fracturing force. Some ten years ago I reported a case in which a man was struck by a locomotive moving at such a speed that he was carried along in front of it for a distance of forty feet before he was flung over to the other track, receiving, among other instantly fatal injuries, a fracture which completely separated his skull into two portions, an anterior and a posterior. Between such terrific violence as this and the blows or falls which are the usual causes of fracture of the skull, there are, of course, innumerable gradations, with corresponding influences on the lesions produced.

Dr. Dulles said: It would not be possible to prepare a paper of the kind which I have presented this evening in such a way as to meet in advance all the objections which might be raised in regard to it; neither would it be possible in the time at our disposal for discussion to answer all such objections. I would only say briefly, that all that is claimed for the "bursting theory" is, that it seems to account for a very large number of indirect fractures. Experience alone can deter-

mine the reliability of any theory, and, while I have become convinced of the correctness of this one, time will decide the matter.

In answer to the objection raised by a Fellow, I would like to say that it is not really an objection, but rather supports the "bursting theory." The fractures passing horizontally, of which he speaks, are just as much meridional as though they passed vertically. As I show you on this skull, any line passing round the surface in the shortest direction from pole to pole—that is, from the point of impact to the antipodal point—is meridional, and the line passing horizontally is as much meridional as any.

Finally, in regard to the case about which a question has been asked, I would say that I do not now recall a single instance of the many which I have investigated, in which a stellate fracture was not a direct fracture; and I believe such an appearance is an indication that the fracture was caused by violence applied at that very spot. This violence, of course, may be due to a blow on the point where the fracture appears, or to a fall in which the head strikes some resisting body at this point.

NOTES OF A CASE

OF

(I.) RAYNAUD'S DISEASE, AND (II.) OF GANGRENE COMPLICATING DIABETES MELLITUS.

By

J. H. MUSSER, M.D.,

PHYSICIAN TO THE PHILADELPHIA HOSPITAL AND TO THE MEDICAL DISPENSARY OF THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA.

[Read March 3, 1886.]

(I.) Raynaud's Disease.

Several names have been proposed or terms applied to indicate the pathological and clinical phenomena which were so graphically portrayed in the following case. It belongs to that interesting group of diseases of the nervous system in which vaso-motor and trophic disturbances are paramount. As will be seen, in all probability the nutritive changes resulting in gangrene were probably due to causes extraneous to the nervous system. There were present in addition, however, marked vaso-motor phenomena, and if with Dr. Barlow, of London, we consider that the paroxysmal character of the circulatory disturbances is the essential clinical manifestation of Raynaud's disease, then the term is a

fitting one to apply to this case. As may be implied from the above, and as the history will show, the clinical appearances and pathological changes were complex. What immediately concerns us, however, was the presence in a high degree of the vaso-motor disturbances known as local syncope and local asphyxia.

At the time this case was under observation, and when we were deeply involved in a study of the known facts of this singular disease, Dr. C. L. Dana[1] treated of it in a very able and exhaustive article, entitled, "The Acro-neuroses." This term he used to include all the functional affections of the extremities, and under the subheading of "Vaso-motor and Trophic Disturbances," he included the group of cases of which this is an example. It would be supererogatory, therefore, to traverse the same ground, and we will, at the risk of some repetition, however, after presenting some few facts, be content with a report, we fear too wearisome, of our case, with such remarks as its special features may suggest.

Much difficulty has attended the selection of a name to characterize the *ensemble* of symptoms which is presented in these cases. Names, it is true, are applied which vividly signify particular stages. What more striking than "dead fingers," digiti mortui, or local syncope? And the same may be said of local asphyxia descriptive of the cyanosis, though possibly from it an improper conception of the pathology of the disease may be formed. Symmetrical gangrene tersely describes the terminal anatomical changes, but it is not, as Southey suggests, a strictly correct term, for often gangrene is

[1] New York Med. Record, July 18 and 25, 1885.

absent and quite often it is unilateral only. Erythromelalgia has been suggested, and Dr. Dana thinks Mitchell and Raynaud independently described the same affection, but it appears, and if the statement is not correct we hope to be corrected, that Dr. Mitchell, who was familiar with Raynaud's work, describes states in which the bloodvessels were generally dilated, and pain was a most marked antecedent and concomitant feature.

Raynaud's disease should be applied to that vasomotor affection which is characterized by tonic spasm of the bloodvessels causing local syncope, local asphyxia, and gangrene. In local syncope the parts affected are "dead," numb, cold, pallid. In local asphyxia the parts are blue or mottled, lower in temperature than normal, and the seat of intense, burning pain. Both of these circulatory disturbances may be attended by diminished tactile sense and local sensibility, and they occur in paroxysms, which may terminate either in the restoration of the parts to their normal condition, or the one (local asphyxia) may end in scleroderma (Grasset, Hallopeau).[1] These phenomena occur not only in paroxysms, often in quick succession, but also are marked by distinct intermittency, and the entire disease is frequently characterized by pronounced remissions. Gangrene occurs only as a sequence of the conditions just described, and may be described as dry gangrene or mummification, as the form resembling frostbite, and in dry, hard plates or parchment metamorphosis of the

[1] See also Finlayson, "On the Occurrence of Symmetrical Gangrene of the Extremities in a Case of Scleroderma Adultorum." The Medical Chronicle, 1884-85, 1. 315. Local syncope antedated and accompanied the scleroderma; the gangrene complicated it, causing death.

dead tissue. It is generally attended with much pain, but is not the cause of septic complications or of death (Southey).

Local syncope, be it remembered, frequently occurs independently of sequential changes. Many cases have lately been recorded; among the most interesting ones those of Richardson,[1] of London, may be mentioned. It may be due to peripheral impressions as seen in the action of cold, or to central functional excitation as in a case of general hysterical paralysis, related by Mitchell,[2] in which there was spasm of the vessels of the left leg, so that it became white and cold. Another interesting case is recorded by the same observer, in which, after injury to the nerve of one leg, the unaffected member became "dead."[3]

Local asphyxia likewise is observed alone in all grades, from the mottling that is seen in delicate children exposed to cold, and the mottling which Cavafy[4] described as pathological to the cyanosis that attends heart disease or goes with scleroderma.

The grouping together of these vaso-motor phenomena, their occurrence in symmetrical parts of the body, and in the sequence stated above, are, however the essential features of Raynaud's disease. In addition to other features of this curious disease, it may be stated that it occurs most frequently in females, at an early age, and after a previous debilitating disease or in the course of some diathetic ailment. The origin of many cases has been attributed to malaria, while three cases are recorded

[1] Richardson, Asclepiad, Jan. 1885.

[2] Mitchell, Diseases of Nervous System. Phila., 1885, p. 32.

[3] Mitchell, Diseases and Injuries of the Nerves.

[4] Cavafy, Trans. Clin. Soc. London.

as occurring in the course of diabetes.[1] The duration of the disease varies from a few weeks to years.

One of the most striking characteristics is the intimate relation of these vascular and trophic changes in the extremities to other affections of the sympathetic nervous system. These allied affections have been remarked by Barlow,[2] and he calls attention especially to paroxysmal hæmatinuria. Paroxysms of this latter disease alternated with the limb disorder in one of his cases, and they were fundamentally similar in that both occurred intermittingly, were worse in warm weather, rarely if ever occurred at night, and were ushered in by yawning and vomiting.

Hutchinson[3] recorded a case in which iridoplegia was associated with local syncope, asphyxia, and gangrene of the ears. The patient was a female, aged thirty, who could not resist cold. The pupils were partially dilated and absolutely fixed. Recovery followed. More curious still is the case of Weiss.[4] The disease was seated in the extremities and was of many months' duration. During its course temporary aphasia occurred twice, which he believed to be due to an ischæmia of the speech centres. There was no vascular or organic nerve disease. You will recall that Raynaud observed contraction of the central artery of the retina in one of his cases, and attributed it to vaso-motor influences. Klein and Scetlin prove, however, to the satisfaction of Landois, that this artery is not influenced by section or stimulation of the sympathetic.

[1] Barlow, Raynaud, Fox.
[2] Trans. Clin. Soc. Lond., 1879, xv. p. 167.
[3] Hutchinson, Med. Times and Gazette, 1871, ii. p. 678.
[4] Weiss, Zeitsch. f. Heilk., 1882, vol. iii. p. 233.

In one of Southey's[1] cases there was joint involvement to a high degree, and the case looked not unlike a rheumatic purpura. He attributed the joint changes to nerve influences however, and cases are recorded of swellings, induration, and contractions of the joints. Hutchinson sees in these cases a possible explanation of the pathology of "end joint" arthritis.

Vascular and trophic skin changes also alternated with or accompanied the disease in the extremities. Among others may be mentioned urticaria, erythema nodosum, and eczema.

A word as to the pathology. There is no doubt the phenomena are due to a spasm of the bloodvessels. This spasm has been attributed to central organic diseases or functional excitation, to excitability of the sympathetic ganglia along the vertebra or to those on the bloodvessels, and finally to degeneration of the peripheral nerves.

The latter possibility finds an advocate in Pitres,[2] who found in cases of symmetrical gangrene extensive peripheral neuritis with degeneration of the nerve fibres. On the other hand, Semmola found degeneration of the central vaso-motor nuclei, as well as the vagus, in a case of functional heart disease and asphyxia of the extremities. No other evidence has been furnished either of peripheral or central degeneration, and the theory of reflex spasm receives the most support. Raynaud believes there is great exaggeration of the irritability of that part of the central gray matter which presides

[1] Southey, Trans. Clin. Soc. Lond., vol. xvi. St. Barth. Hosp. Rep., vol. xvi.

[2] Pitres and Vaillard. Archiv. de Physiol. norm. et path., 1885, No. 1, pp. 106–127.

over vaso-motor innervation and, hence, any slight peripheral stimulation reflexly causes spasm.

Webber would have us believe that the reflex arc is completed on the vessels themselves, and many others teach that spasm occurs from the local irritation of sympathetic ganglia or filaments connected with the vessels by a poisoned blood. Especially, they say, these phenomena are presented if the heart is too weak to overcome the peripheral spasm. This theory is similar to the one which explains the vascular phenomena of Bright's disease. The localization to the extremities of the vascular phenomena in Raynaud's disease is due to the exaggerated local excitability of the sympathetic ganglia.

Some speculations have been advanced to explain the phenomena of syncope and asphyxia. Local syncope has been attributed to slight reflex spasm of the vessels, local asphyxia to a profound arrest of the circulation. The former is due, some hold, to arterial and venous spasm, the latter to arterial spasm alone. Again, Nedopil believes that local asphyxia is due not only to an arrest in the supply of arterial blood as in syncope, but also to capillary stasis of the venous blood. With these preliminary remarks we prefer to present the history of a case that exemplifies some of the interesting features just detailed.

CASE.—One of the peculiar features of the patient whose history I am about to detail, was a great tendency to blushing. From early childhood he was known to be shy, and this mobility of the vasomotors was excited on the slightest provocation. Another, was his susceptibility to cold; so marked that he would not indulge in the usual winter sports of his boyhood days. On account of this sensitiveness, rather than from excessive exposure, he had an attack of

chilblains five years previously. Then again he was easily affected by tobacco, for, although he had tried faithfully, yet he could not "harden" himself sufficiently to prevent his becoming pallid on occasions, and having repeated attacks of pallor noticeable to his companions, if the smoking was excessive. Finally, this susceptibility of his sympathetic system to impressions may be illustrated by the frequent occurrence, without discoverable cause, of vertigo. About five years ago J. H. had an attack of intermitting fever. This was succeeded by spells of vertigo, which developed at any time, without cause, and were accompanied by blindness. The attacks lasted but a few moments and were relieved by sitting down. The vertigo was subjective and had not been preceded nor accompanied by aural or gastric phenomena. After the lapse of a year the attacks, which were of daily occurrence, gradually but completely disappeared.

Aside from these circumstances, the medical history of the young man is uneventful up to the time of the present attack. He has never had any other illness, never migraine; never syphilis or gonorrhœa. His habits have been good; he has never indulged in excessive venery. The mother of J. has been insane for five years; caused by prolonged mental strain. A sister has been very hysterical, but otherwise she and her other two sisters have always been well. The same may be said of one brother; of the other, that he is subject to rheumatism. The father was healthy, complaining only lately of cardiac pain and dyspnœa. His maternal grandparents are very well; his paternal grandfather and two uncles died of heart disease.

J. H. came to me on the 12th of July, 1885, with the following immediate history of his complaint. On the 17th of May preceding he went out as milk-server, having previously been a paper-hanger. In addition to the serving of the milk, he had to "ice up" a great deal and also attend to a horse. The handling of the ice was trying to his hands, but did not prevent him working. On the fourth day of his new work a large lump of ice fell on the palm of his left hand, bruising it severely. Five days after this his hand suddenly turned white, and became very cold, numb, and the seat of pain. At the same time he experienced slight tingling and numbness in the right hand. The paræsthesia of this member disappeared

in about a week. The pain in the left hand was lancinating and prevented sleep. He continued at work, however, but it was interfered with by the pain, numbness, and stiffness.

Nearly five weeks after the receipt of the injury (July 1st) he was awakened by a severe pain in the hand and on inspection found its color had changed to a dusky red. The pain continued not only in the hand, but was also felt along the brachial artery. A short time before this change he noticed slight blueness of the finger-tips.

On the occasion of his first visit the following appearances were observed:

The hand is very cold and the coldness extends up the arm to the elbow. The surface temperature is lower than my thermometer will register. On the dorsal surface of the hand there is a general venous or dusky redness extending one inch above the wrist-joint. The blueness is deeper toward the tip of the finger. The index finger is markedly cyanosed. All the nails are blue. The little finger is also quite blue, the middle finger less than the other two. The last phalanx of the index finger is blue-black, increasing in depth of hue from the median line to the sides as the palmar surface is approached. The thumb is similarly discolored. Pressure on the skin causes pallor, which instantly on removal of the pressure changes to red, the wave of color beginning in the periphery. On pressing the fingers, instead of pallor, a bluish tint of the anæmic skin is observed. Here the color changes are more rapid. The skin of the fingers about the joints is wrinkled. The cyanosis is deeper on the finger-tips. The hue is of equal intensity on the little and index fingers, the thumb is the next deepest, the middle the next, and the ring the least discolored. The more marked hue of the first two fingers indicated does not give change to pallor on pressure, and that of the thumb and middle finger but slightly. On the index finger, over the terminal phalanx, one-quarter of an inch from the extreme end is a small sphacelus the size of a bean; its boundary is irregular and it is surrounded by an area of redness. A deeper colored spot is seen nearer the joint of the same surface. Outside of the area of congestion an anæmic line two lines wide is observable. The darker slough is said to be three weeks old. A smaller sphacelus is seen

on the little finger. From the wrist almost to the elbow the arm is quite red. *Pain* is experienced in the finger-tips and is excruciating at nights. The ends of the fingers are very painful on pressure. While taking the above notes the entire hand to the wrist in repeatedly succeeding waves changed from the cyanotic hue to a deep pallor: the finger-tips alone remained blue. This pallor lasted five minutes and then changed again to the original color. These changes take place four or five times a day, and the pallor continues from five to fifteen minutes. Accompanying them there is extreme pain in the tips of the fingers, but no throbbing. Sensation is not lost in any part of the hand except on the sloughs: impairment of the tactile sensibility is marked. There is no loss of power and no change in the electrical reactions. The hand is extremely sensitive to the heat of the atmosphere, extreme pain being experienced in the middle of the day if exposed to the sun. When cyanotic, the sensation to the hand placed in cold water is of heat: in hot water the same sensation is felt, cold applications increase the pain. As previously noted, the pain is worse at night, keeping him from sleep, and is especially severe in the recumbent posture. There is no atrophy of the arm or hand, no œdema. The pulse in the radial is absent and pulsation can scarcely be felt in the brachial and axillary arteries. The nails grow, but the patient is unable to cut them on account of pain.

His general health is good. He sleeps poorly on account of the pain, but there are no cerebral symptoms. Vision normal, eye ground healthy, pupils dilated but movable. His appetite has been poor for the past seven weeks, digestion good and bowels regular. The heart is normal and free from palpitation. The urine is light in color, normal in quantity, acid in reaction, and free from albumen and sugar, but contains some urates.

July 16. Daily applications of the faradic current have been used, morphia taken at night, and $\frac{1}{100}$ of a grain of nitro-glycerine three times daily. The electricity relieves the pain, and the cyanosis is not so great save in the index finger, which is more blue. No increase in size of sloughs; fingers are shrunken so that the skin can be pinched in folds from the tip to the first joint. Local syncope occurs much more frequently and continues much longer: instead of five times, it takes place twenty-five times in the twenty-four hours.

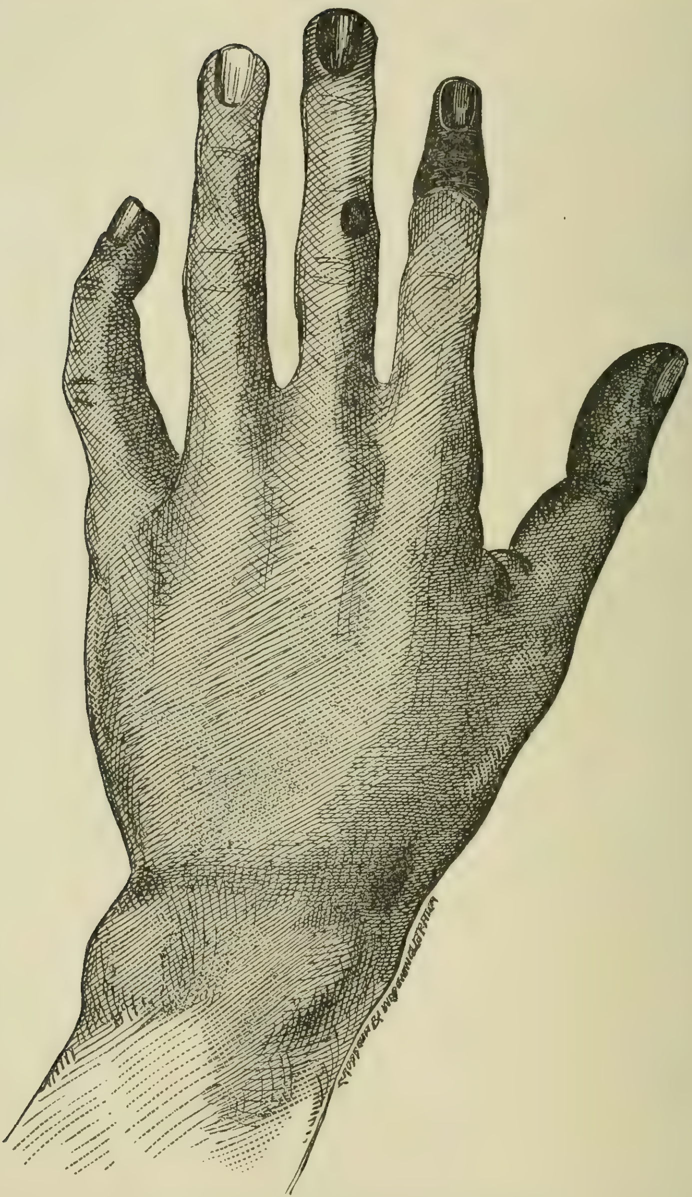
After pressure the capillaries do not fill so quickly as before. The pallor is of a different shade, instead of dead white it is now yellowish-green, not unlike the yellow tinge after a bruise. Pain is complained of, especially on the radial side of the middle and on the opposite side of the index finger. The middle phalangeal joint of the index finger is more painful than formerly. The hand is stiff; the grip is not so strong as it was.

21st. Shrivelling of the extremities of the index and ring fingers is noticed, and hence the epidermis is loose. The ring finger is free from asphyxia; the middle finger is improved; the index finger is blue-black, the color not changed by pressure. Pain and tenderness of the second phalangeal joint of that finger continues. On the radial side of the second joint of the middle finger a slough is forming. Index finger still quite stiff; thumb asphyxiated but not shrivelled. Pain continues: heat of sun increases its intensity, and hence he remains in the house during the day. The solar heat turns the hand purple, he says. The loose epidermis is insensible, but firm pressure causes pain in the finger-tips.

24th. Since the last note an improved and marked change has taken place. The hand does not turn pale as often as it did, and its general warmth is increased, but the redness continues. The second joint of the index finger remains swollen and tender, and the ecchymosis on its surface remains. On the finger-tips the most marked change is seen. The palmar surface of the last phalanx of the index finger is pale and hard as if dead, and on its centre the brown ecchymosis is seen. The asphyxia has deepened very much on all the fingers, and the pain and tenderness are increased. About over the first joint of the index finger a line of demarcation is beginning to form.

26th. A thin, brown fluid has been oozing through small orifices in the raised and redundant skin of the index finger. This epidermis has become brown. Lymphangitis to the elbow from the index finger is observed.

29th. Lymphangitis has subsided; the discharge of ichor ceased and the formerly loose epiderm has become intimately adherent to the finger, which is dry, hard, and insensible from the tip to three lines from the first joint anteriorly, and at the sides to the root of the



The figure shows the extent of the gangrene of the forefinger. The outer dark spots represent the various small sphaceli; July 29.

nail. The gangrene extends under the nail. The nail is black, and from the root of it to the palm the skin is asphyxiated. The fingers remain stiff, the skin of the thumb is hard and a little purpuric spot is seen on the tip. Similar spots are seen on the dorsal surface of the middle finger as previously described. The asphyxia of the middle finger is less, but the second joint is painful and purplish on its surface. At the root of the nail a small slough is forming. On the palmar surface of the first phalanx of the little finger a dark brown and loosened cuticle is seen; it is a little tender. Pallor has not been observed for two days. Hand not as cold as formerly.

August 1. The gangrene has extended a little below the first articulation on the palmar surface of the index finger, and around the finger to one-eighth of an inch below the root of the nail. The nail is very black. A line is forming, and the finger is offensive. The fingers pain if the hand is held up.

12*th*. The dry gangrene has continued to extend on both surfaces of the finger to the first phalanx: the remainder of the finger is much swollen and indurated. Sloughs on the palmar tip of middle and ring fingers continue, and the ecchymosis continues around the nail on the palmar surface. The subcuticular parts have atrophied so that the cuticle covers it like a shell. Same condition, though not so marked, on the thumb and little finger. The nails have grown, but as yet he has been unable to cut them on account of the severe pain.

16*th*. Line of demarcation forming. The finger still blackened.

21*st*. Index finger thickened and swollen; gangrene now extends to the bone. Epidermis is still raised over the tip of the middle finger which is hard and painless, and probably underneath new skin has formed. Thumb side of index finger is very black along the nail; tip of little finger still red. The general cyanotic hue of the remainder of the hand has almost entirely disappeared; instead thereof there is an irregular mottling. The temperature of the hand is below normal, but not as cold as formerly.

24*th*. After a complete line of demarcation had formed, Dr. Dulles removed the affected tip. The removal of the gangrenous portion served to free the patient from pain; on account of which, he

was enabled to sleep at night in the recumbent posture. Succeeding the operation the cyanosis of the entire hand disappeared entirely.

September 15. Dr. Dulles had found it necessary to remove not only the dead portion, but also a small portion of the living tissue. The stump healed kindly and the patient has just returned to his work.

January 11, 1886. It was noted that the nails grew but slightly while the patient was under observation. It is now seen that their nutrition had been seriously interfered with, the structure that grew then being different from the portion toward the end, or the new healthy portion at the matrix. The diseased portion, seen on all fingers except the ring, is from one-eighth to one-fourth of an inch long, slightly discolored, friable, and rough, and on a lower plane than the older and newer portion, due probably to the antecedent shrinking of the finger-ends. The hand to the wrist is very red, and the vessels are emptied readily by pressure. The right hand is also quite red, but not so markedly as the left. The pulse has returned, but feebly, in the radial and brachial. He suffers some pain along the vessel. The hand in a warm room is of the same temperature as the right, but on exposure it becomes cold more quickly, and with more suffering.

Remarks.—The suddenness of the onset of the symptoms in this case, with the absence of pulse in the radial, brachial, or subclavian artery, appears to show that primarily, at least, there was an arterial thrombosis. The origin of it is obscure. It is possible that an arteritis had been set up by the blow in the hand, or even by cold (ice), and the thrombus developed secondarily. Dr. Piersol very kindly examined a portion of the healthy tissue that had been removed, and reports that he could find only small vessels filled with clotted blood but without inflamed walls. Recently J. H. has reported, complaining of pain behind the clavicle. There is no disease in the axilla, and nothing to account for a central thrombus. It is, therefore, impossible to find

the cause of the vessel obliteration. There is one case alone, of many cases of symmetrical gangrene reported, in which a large vessel did not pulsate. I hesitated, therefore, to account for the absence of pulsation by arterial spasm alone, and yet it would not be unreasonable, for as we can have abnormal pulsation of a vascular trunk so can we have abnormal spasm.

As hinted in the beginning, in all probability the gangrene was due to the vessel obliteration, intensified, however, by the occurrence of local asphyxia. If the obliteration of the vessel be due to causes other than spasm, the phenomena of syncope and asphyxia from capillary spasm can be explained by invoking reflex action. That is, the clot in the vessel or the source of the clot in the thorax, acts as an irritant to peripheral vaso-motor nerves through the "pressor" fibres, causing reflex capillary spasm.

A neuritis, on the other hand, may be conceived as the cause of the vascular and nutritive changes. Yet the absence of paralyses and atrophies, and the electrical reactions, together with the result of the case, would point rather to functional than organic nerve lesion.

NOTE.—*History.* In addition to the complete bibliography of Dana, and the references in the text, the following are worthy of study. E. L. Fox, on The Sympathetic Nerve, etc., has a most admirable and exhaustive account of this affection, and quotes some interesting observations of McIntyre and others. A list of authors and reported cases are given. They will be excluded from this list. Ross, Wilkes, and other works on the nervous system may be consulted. One of the earliest and best cases recorded is by A. S. Myrtle, M D., in the Lancet, 1863, i. p. 602, entitled a case of "Anæmic Sphacelus." The books of Woakes on Deafness, Giddiness, etc., and on Post-nasal Catarrh, can be read in this connection with interest.

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FRY, F. R. Raynaud's disease. The Journal of Nervous and Mental Diseases, Jan. 1886.
DANIEL, W. E. Spontaneous gangrene of both feet. Lancet, 1857, i. 425.
CRISP. Brit. Med. Journ., 1883, i. 572. Spontaneous gangrene, no pulsation in tibial.
Gaz. méd. d'Orient. Constantinople, 1880–81, xxxiii. 36.
LANCEREAUX, E. "Des troubles vaso-moteurs et trophiques liés a l'alcooliques et à quelques autres intoxications chroniques, etc." This author observed among other vaso-motor disorders, in alcoholic paralysis, local asphyxia and gangrene of the extremities. In his dissections he found the spinal cord healthy, but decided evidences of a peripheral neuritis.

(II.) GANGRENE COMPLICATING DIABETES.

In a few words, if only from association with the preceding, this case merits slight attention. Cases of Raynaud's disease have occurred in the course of diabetes. This is a case of simple, dry gangrene, without vascular phenomena, however, and indicates how a slight injury may be productive of serious results in a diabetic.

The portions before you are the last two phalanges of the little finger. The patient, a female, aged thirty-five, has been under observation five years. Six weeks ago, she received a slight cut at the second joint of

the finger. It bled a little, was painless and soon forgotten. A duskiness was observed around it a few days after, and extended to the tip. At the same time the phenomena of dry gangrene presented themselves, and after a long time the line of demarcation had so completely formed as to permit the snipping of the ligaments about the joint to remove the mass. A slight lymphangitis had developed, and now the stump and adjacent structures of the hands are infiltrated.

CASE OF SPINAL CURVATURE THE RESULT OF UNRECOGNIZED ASYMMETRY IN THE LOWER LIMBS.

By

THOMAS G. MORTON, M.D.,

SURGEON TO THE PENNSYLVANIA HOSPITAL.

[Read March 3, 1886.]

I SHALL occupy the attention of the College for a few minutes in showing a case of marked spinal curvature the result of asymmetry in the lower limbs. The history is briefly as follows: The patient is from Wyoming County, Pa., aged twenty-four. He was a puny boy, until about the age of fifteen. He then grew rapidly and gained his height, which is now five feet eleven inches, in the course of fifteen months. He has never been sick in his life, with the exception of the ordinary ailments of childhood. While growing rapidly, he noticed when lifting a stone some pain upon the right side. He has never experienced pain, except when lifting weights, never while lying down. He has been a student of medicine for two and a half years. Sitting on hard benches has caused considerable spinal ache. A year ago the diagnosis was made of serious necrosis of the lower dorsal and lumbar vertebræ. For this, a plaster jacket was applied. During the past four or

five years, there has been a constantly increasing lateral curvature. On examination I found a marked spinal curvature with lameness. Looking at him as he passed in front of me, I noticed great asymmetry, in the length of the lower limbs. This had not been recognized before. I found it necessary to use a block an inch and seven-eighths in thickness, to bring the right limb to its proper length as compared with the left. With this elevation, the spinal curvature almost entirely disappeared, careful measurements also confirmed the shortening of this limb. It has been a little difficult to determine whether, at the time he commenced to grow more rapidly, there was any involvement of the spinal nerves which impaired nutrition of the affected side, which involved not only the upper part of the body, but the lower part as well. My experience in lateral curvature is, that in the absence of asymmetry of the lower limbs almost all curvatures arise from some morbid condition of the spine, as seen in infantile paralysis. There is generally more or less muscular atrophy and permanent deformity in cases of marked lateral curvature. This is seen in the diminished size of the upper and lower extremities of the affected side in most of these cases. It is barely possible that when this patient commenced to grow, at the age of fifteen, development did not take place in the right side and lower extremity, but against that theory examination shows that the thigh and calf muscles are equally developed, the only variation is the unequal lengths of the limbs. I, therefore, believe that this want of development is congenital. The patient has always had difficulty with the heel of the shoe upon the right side, which would wear out differently from that of the other.

He now has no spinal tenderness, except when in the deformed position. He is now unable to stoop very well, but before the application of the plaster jacket, less than a year ago, all of the spinal movements were normal and he was quite a gymnast. He ascribes the present condition to the use of the jacket. The unnatural pressure perhaps led to a partial absorption of the intervertebral cartilages upon the right side, and this may have caused more or less fixation of the vertebræ.

In many cases asymmetry in the lower limbs produces a slight amount of lateral curvature with pain. A careful measurement of the limbs will detect this at once. I have seen a number of such instances.

[After the reading of the preceding paper:—]

Dr. W. W. Keen said: It seems to me that, in standing with his foot on the block, the patient's right buttock was higher than the left. This appears to be confirmed by examining the line of the anterior superior processes of the ilium. The deformity appears to be over-corrected by the block one and seven-eighths inches high, and the shortening less, therefore, than was stated. I should like to ask Dr. Morton whether there is any explanation with reference to the causation of the trouble.

Dr. De Forest Willard said: If this be simply a case of lateral curvature, how does Dr. Morton account for the marked rigidity? Rigidity is rare in lateral curvature except in the late stages. It is difficult to determine in this case whether rotation has occurred, on account of the inability of the man to stoop. The pressure of a gypsum jacket for so short a time should have produced relaxation and atrophy rather than stiffness.

Dr. Thomas G. Morton, in conclusion, said: I think that the want of symmetry in this case is congenital. With reference to the slight overcorrection of the deformity, it may be that this block is a little high, but the only way we have of noting when the asymmetry is overcome is by the eye, and the absence of lameness in walking.

Lateral curvature of the spine frequently occurs in hip-joint disease. One reason that it does not occur in many cases is that the shortened limb is provided with a proper shoe apparatus. I did not say that absorption of the intervertebral substance had occurred, but I think it possible. I think it probable that in the course of six months or a year the now slight unrectified curvature may disappear. In order to support the spine in every direction, I have the patient supplied with an ordinary spinal brace.

As I have said, the spine was perfectly pliable until the plaster jacket was applied and worn a number of months. This may have had some influence in stiffening the spinal column. The patient has never had any symptoms indicative of Pott's disease.

THE ANALGESIC ACTION OF THEINE.

By
THOMAS J. MAYS, M.D.

[Read April 7, 1886.]

Nearly a year ago I essayed to investigate the physiological action of theine, caffeine, and guaranine, principally in relation to the nervous system, and from the experimental results which I then derived, and which were published in *The Therapeutic Gazette* for September, 1885, I was led to believe that, while all these agents had a decided affinity for the nervous system of the frog, caffeine chiefly affected motion, and theine and guaranine mainly influenced sensation. I carried this principle of the action of theine into practice, and found that this theoretical deduction was verified by its power of promptly relieving painful affections of the human body.

These clinical observations on theine appeared in *The Medical News* for December 12, 1885. At that time I was under the blissful impression that these alkaloids were manufactured separately from tea, coffee, and guarana, but soon found that this delusive charm was only a snare—that they were neither the one thing nor the other, but that they were made indiscriminately from Chinese tea, Paraguay tea, kola-nut, coffee, or guarana; and were labelled promiscuously to accommodate the

demands of the trade. This singular state of affairs has brought about a remarkable complication in the market. The great bulk of what is supposed to be caffeine, is not caffeine at all, but theine; since under all ordinary circumstances it is much cheaper for the manufacturer to work with tea, than with any of the other raw materials.

On this subject, Dr. Charles Rice, than whom there is no better authority, in an editorial in the March number of the *American Druggist*, says:

"We have reason to believe that neither theine nor caffeine is manufactured in this country, but all that is sold is imported from abroad. . . . We translate the following passage from Von Gehe & Co.'s *Handelsberichte:* 'Although *caffeine* is in fair demand, and still remains at its comparatively low figure, an advance is to be expected, because the proper crude material for its preparation, namely, the *cheaper grades of tea*, have been for some time unobtainable.' . . . From this it appears that cheap tea leaves are the principal source of caffeine, and, therefore, also of theine.

"We are, moreover, assured by the house of E. Merck, of Darmstadt, that in view of the heretofore acknowledged and undoubted identity of caffeine and theine, manufacturers consider themselves at liberty to choose whichever crude material happens to be cheapest in the market. A lot of coffee, damaged by water or otherwise, would probably sometimes form a cheaper source than tea leaves. Usually, however, the latter are used as the source."

The opinion of Dr. Rice, that no theine or caffeine is manufactured in this country, I can confirm so far as this city is concerned; for inquiry among the principal dealers and manufacturers of drugs has failed to reveal any who did not import these alkaloids.

In all fairness it must be admitted, however, that the manufacturers were justified in producing and mixing

these drugs promiscuously, since they are taught that they are identical both in chemical constitution and in therapeutic action, and if this is true, no earthly reason can exist for their separate manufacture. But it is very evident, that for the purpose of inquiring into the truthfulness of their supposed identity, it is idle to test them in a state of combination; hence, in so far as my former experiments are to indicate any differential action between these agents they are valueless. There can, however, be no doubt that what I used under the name of theine was principally or wholly theine, but the nature of the substance which I regarded as caffeine is more uncertain, yet it must have contained something besides theine, or else I could not have obtained dissimilar results from the two substances. In this connection I would also state that neither the theine nor caffeine which I then used came directly from Merck, as I had supposed at the time, and erroneously stated in my first article.

In view of this chaotic condition of things, I determined to review all my previous work, so far as it related to theine and caffeine, and my thanks are due to Mr. William Harris, with the firm of Messrs. Henry C. Blair's Sons, for isolating these alkaloids from tea and coffee respectively, and for thus enabling me to test each one in its separate state. I have thoroughly gone over my former ground, and have found ample proof to substantiate my previous investigation in most essential points, and even have found reason to believe that the difference between the action of the two in regard to sensation is greater than my earlier experiments indicated.

Since tea and coffee are believed to be very different

in their gross effects on the human body, there is no obvious reason why their alkaloids should be considered identical in their action, unless, after being shown that they are chemically alike, the assumption, of course, naturally follows that they are alike in their physiological action. This idea has also been encouraged by experimental physiologists, among whom are Aubert, Albers, Amory, Bennett, McKendrick, Burnett, and others, although Leven, who, as early as 1868, experimented with the separate alkaloids, which it does not appear the other above mentioned experimenters did, found that theine produced convulsions in frogs, while caffeine did not. This fact my experiments confirm, and must be regarded as an important indication as to the manner of their action.

A detailed account of my experiments just completed will not be given here, since they would swell this paper to inordinate dimensions; but their condensed results, which appear below, show that, while the actions of theine and caffeine agree in many respects, they also disagree in some important particulars. They both first affect the anterior extremities of frogs, and both diminish respiration and cause hyperæsthesia during the latter stage of the poisoning process, although the supersensitiveness is much more marked in theine than caffeine. They differ in the following respects: Theine principally affects sensation, while caffeine principally influences motion; theine produces spontaneous spasm and convulsions, while caffeine does not; theine generally produces muscular relaxation in the posterior extremities, while caffeine produces muscular rigidity in the same; theine impairs the nasal reflex early in

the poisoning process, while caffeine does not, if at all, until in the very last stage.

It may be remarked here that since winter frogs, which are not so irritable as summer frogs, were used in my last experiments, these results, so far as the induction of hyperæsthesia is concerned, may be modified to some degree; but, so far as sensation is concerned, which is the main point of interest here, I am confident that the distinction made above between the action of theine and caffeine rests upon a fundamental basis. It would be erroneous, however, to suppose that the action of theine is strictly and exclusively limited to the sensory nerves, and that of caffeine limited in the same way to the motor nerves, for both my former and later experiments teach that these are merely their strong characteristics, although one may supplant the action of the other in a minimum degree.

On man, theine can only be considered a poison in large doses. Frerichs, Lehmann, Husemann, and others drank it, and found that in doses of from one-fourth to one-half of a decigramme, it produced "frequency of the pulse, irritation of the bladder, cerebral excitement, slight hallucinations, and lastly, a desire to sleep." Dr. C. H. Castle, of Cincinnati, Ohio, was induced to test the action of theine by the appearance of my article in *The Medical News*. The results of his experiments, which were both physiological and clinical, were read before the Cincinnati Medical Society, and published in *The Cincinnati Lancet and Clinic* for February 6, 1886, and since they were made with pure theine, extracted by his own chemist, and because they add some hitherto unobserved features of the drug, they are very interesting

and instructive. He made, in all, three experiments upon himself, and of these he says:

The first time I injected one-sixth of a grain into my left forearm, immediately below the flexure of the elbow-joint. I will not weary you with the details of the observations, as made at intervals of a few minutes. The noticeable effect was remarkably rapid. An angry blush and swelling spread from the point of injection, but what looked like a most delicately tender spot was singularly devoid of sensation. It was not, nor did it become, totally anæsthetic, but, in what appeared to be an inflamed area, sensibility was markedly dulled. Presently a numbness such as we feel when a member is beginning to "go to sleep," from pressure upon its supplying nerves, stole over my wrist and hand. Little tinglings, scarcely noticeable had not one been on the watch for strange manifestations, came and went in various places. At no time was mobility impaired. The temperature of the left hand, and I beg to call your attention to this point, was markedly lowered. The effects of the injection lasted about two hours.

The second injection was made under somewhat different circumstances. I had rather severely bruised my right hand, and the pain on pressure over the metacarpal bone was quite severe. I therefore injected one-third grain of theine subcutaneously, over the ulnar nerve just below the elbow.

The same phenomena were noticed as in the first use of the alkaloid—sensibility around the injection was markedly diminished, lower down the forearm and in the wrist and hand less markedly so; the right hand lost as much in temperature as had the left hand previously, but I am compelled to say that I was disappointed in the analgesic effect that I had expected. The extreme sensibility was dulled, to be sure, and my hand no longer hurt when I put it into my overcoat pocket, as it had before, but any deep pressure instantly informed me that the nerves about the joint still resented the impact of a foreign body with the surface.

The third observation was made again upon the left arm: this time one-half of a grain was used, and though an accident perhaps somewhat marred the experiment, some points were brought out more

prominently. The injection, made for me by a friend, was a deep one—intermuscular—and the withdrawal of the needle was followed by a slight hemorrhage, sufficient to raise a conical subcutaneous tumor of a diameter of a one cent piece. Instantly there was a deep-seated pain, probably due, I thought, to extravasation from the wounded vessel into the intermuscular planes. Full extension was impossible; partial extension very painful, the painful seats being deeply under the point of injection and the dorsal aspect of the metacarpo-phalangeal points.

As in former observations, sensibility was affected almost immediately, and in three minutes the left hand was blanched and almost cold to the touch. My pulse was always full, strong, and but slightly more frequent than normal. Very soon peculiar constitutional disturbances appeared, and not entirely like those described in the standard works on materia medica. I was excited and talkative, and so rapidly did I talk, that I would soon exhaust a subject broached by others, and endeavor to introduce some natural descendant of the idea as a topic of conversation, apparently so far ahead of its proper sequence in a well-ordered train of thought, as to appear like an interruption with an irrelevant subject. Failing to gain a hearing, as I frequently did, I would indulge in snatches of song or broken ejaculations which had a world of meaning in them then, but which, as I look back upon them now, seem utterly absurd.

Alternating with states of great bodily activity were spells almost of a fainting character. These were seven or eight in number, and beginning soon after the injection, gradually ceased after the lapse of six or seven hours.

Dr. Castle gives a very good description of the theine effects as he observed them on himself, and I would say that the only way in which I can account for the decided cerebral intoxication in his last instance, is on the score that unfortunately the deep injection which he practised chanced to introduce the theine into a bloodvessel, and it was at once carried to the brain and produced the hallucinations as described. This is all the more prob-

able because the central disturbance manifested itself very soon after the injection, which, ordinarily, should not have appeared until in a later stage of the poisoning process. The dose which he used, I should now, after considerable experience with the drug, call a moderate one—for I frequently inject from three to four times as much as he did—not deep, however—and I have yet to observe the first untoward constitutional effect.

The physiological effects of theine as brought out in a number of individuals, under my own observation, may be summed up as follows: Dose, one-third of a grain. Numbness of arm and hand below seat of injection, "a feeling as if the hand had been steeped in a solution of carbolic acid," as one of the subjects of experimentation expressed it. A feeling of coldness and an occasional disturbance of the temperature in the member under its influence. A slight reduction in the pulse-rate, and no intoxication of the brain. No impairment of motion. The anæsthesia comes on in a very few minutes after the injection, and was much more marked in some individuals than in others. The temperature disturbance was not regular. Out of four cases in which the temperature was taken, it showed no difference in one, and in another one the uninjected hand was slightly higher (0.4° Fahr.) in temperature than the injected one. In two there was quite a marked fall in temperature—one 0.8° Fahr. and the other 1.2° Fahr. lower in the hand of the injected arm than in that of the uninjected one. And strange to say, the hand which showed the greatest depression in temperature, experienced the least anæsthesia. A certain feeling of coldness always accompanies the numbness of theine, yet, in my earlier observations I failed to detect any difference with

the thermometer. Dr. Castle was the first to notice this feature, and I can quite agree with him, that if the lowering of the temperature is constant after its injection, this will add materially to the clinical worth of the remedy.[1]

It is evident now, I think, that experimentation points out that in theine we have an agent which has the power of paralyzing sensation without affecting motion, with a great degree of accuracy, and this with no appreciable injury to the part which is influenced. It now remains with clinical medicine to decide how far this property can be utilized in practice. Evidence from various sources will, however, be offered here, which seems to show that it has already demonstrated its fitness to fill a most useful position in the treatment of pain. The following cases, the sources of which will be indicated during their relation, have all been gathered from different medical practitioners who took sufficient interest in this drug to test its action, and who were kind enough to transmit their reports to me; and I desire to thank these gentlemen for the scientific and liberal spirit which they evinced in undertaking to investigate a hitherto untried remedy. Both the successful cases and the failures will be given, in order to form an intelligent verdict in regard to its proper place in the treatment of pain.

[1] After this paper was written, I learned from Dr. L. Wolff, of this city, that Dr. A. Eulenberg, in his "Hypodermatische Injection der Arzneimittel," describes the analgesic action of caffeine when administered subcutaneously. On referring to the work, I found that this eminent author had used caffeine for the relief of pain in three cases—two of cephalalgia, and another of occipital pain—and, although he was unsuccessful in relieving the pain, at least permanently, he observed that caffeine produced a local anæsthesia at and in the vicinity of the injection.

The first series of cases were reported by Dr. H. C. Fegley, of Ashland, Penna., who writes:

Since your article on the action of theine was published, I have had the opportunity of testing its analgesic effects in three very aggravated cases of sciatica. In two cases complete recovery, with the exception of slight stiffness of the leg in one, took place. The third was an aggravated case of three years' standing, and although not cured, was so much benefited that she is able to attend to her household duties.

CASE I.—S., December 20, 1885, had been unable to leave her chair, in which I found her, for two weeks, the least movement causing the utmost agony in the whole left leg. When perfectly quiet no pain was felt except occasional sharp twinges shooting down to the foot. I put her to bed and injected a quarter of a grain of Merck's theine into the thigh, about the seat of greatest pain. The injections caused intense burning, which lasted about three minutes, when she said it had left her and that movement in her limb caused less pain. The following evening I injected half a grain, and repeated the dose a day after, when she felt, as she expressed it, entirely cured.

CASE II.—H., January 10, 1886, complained of intense deep-seated pain in the leg, which had been coming on gradually for nearly a week. She never had sciatica before, and two injections of three-quarters of a grain each, relieved her entirely.

CASE III.—Mrs. A. has been a sufferer for nearly three years, and has tried a great many remedies with little or no benefit. Theine in doses of a fifth of a grain relieved her only for three or four hours, but three-quarter grain doses gave her a full night's rest. The intense sciatic pain, she says, has passed away, but there are still some soreness and considerable stiffness in the limb.

I found that the dose you first suggested to me was inadequate, so I first tried one-fourth, then one-half, and finally three-fourths of a grain, and am quite well pleased with it. I have only had an op-

portunity of testing the drug in the three cases just cited, but am inclined to believe it to be a true analgesic in aggravated forms of neuralgia.

Reported by Dr. H. Woddrop, of Loag, Pennsylvania.

CASE IV.—M., married, aged forty, was found last December 4th, with considerable pain, of intermittent character, in left leg. For a month previous he had been suffering with occasional twitches of pain, and was now confined to his couch. He also had stiffness as well as œdema in his other limb. He was treated with morphia (*per orem* and subcutaneously), muriate of ammonia, quinine, iron, strychnine, and arsenic until the last of December, when the swelling had entirely disappeared, but there were considerable pain and stiffness left in the limbs. About this time I saw your article on theine, procured some, and injected one-fifth of a grain deep into the calf of the left leg. This caused some pain, and succeeding symptoms were very near your description. Continued internal treatment and subcutaneous injection of theine occasionally. On January 19th changed internal treatment to small doses of phosphorus, beef peptonoids, and milk. On January 23 could move his limbs freely, had no pain, and appeared, excepting debility, a well man. He received in all twenty-four injections, sometimes twice a day. I attribute his recovery in great part to theine, but the constitutional treatment was also very important. I have no doubt that by the use of theine I shortened the case materially. The burning which was occasioned by the injection lasted about fifteen minutes, after which pain in the limbs ceased. The pain returned at varying periods, not as severe after the injections as before, and it always subsided directly after the use of the theine. I believe theine to be a perfect analgesic, and have noticed no narcotic effects from its employment.

CASE V.—Mrs. T., aged between sixty-five and seventy years, a rather fleshy lady, complained of dyspnœa, excessive weakness, heart's action was muffled—the two sounds running into each other. Also had severe pain along the course of the right sciatic nerve.

Injected theine over right sacral region, and left the syringe and solution of theine in charge of an intelligent lady, with instructions to use it immediately on return of pain. Also prescribed digitalis, ferri sulph. exsiccatus, and quinia. At my next visit I found the heart sounds and breathing normal, and sciatica gone. She had received, in all, three injections of theine.

CASE VI.—This was a case of rheumatic gout in which I used theine for the purpose of obtunding sensibility, and it certainly appeared to have considerable influence upon the pain, but, of course, we could not expect any more in this case from it.

To case first I gave your article to read, and he said that his symptoms of numbness, etc., corresponded with those which you describe. The second and third cases, besides the burning incidental to the injection, experienced nothing more than the relief of pain.

Reported by Dr. George S. Gove, of Whitefield, N. H.

CASE VII.—Injury to the hip-joint of one and a half year's standing. The theine was used at first in small and afterward in large doses without any benefit, either alone or combined with morphine and atropine. Nor had morphine or atropine, combined or alone, any influence on the pain.

CASE VIII.—Patient aged forty-five, employed in a creamery. Has had rheumatism. Brought on a pain in his back by lifting a heavy milk can. Worked all that day, had a restless night following, next day was quite lame and stiff; worked that day but was used up by night time. The following night of the second day he had very little sleep on account of the severe pain. Liniments and hot fomentations were applied without effect. When I saw him next morning the pain was so great that in trying to dress himself he fainted and fell to the floor. I gave him one-half grain of theine hypodermatically, about ten inches above the hip-joint, and waited half an hour without affording much relief. I then gave him another half grain injection and the pain lessened. After a short time the pain entirely disappeared and has not returned since.

Dr. William Hall, of Conshohocken, Pa., reports:

CASE IX.—I have used theine in three cases, two of cervico-brachial and one of intercostal neuralgia. The case of intercostal and one of cervico-brachial neuralgia were entirely relieved. In the other case theine failed, as did all other remedies. This last case is a women whose father died of, and whose sister is suffering from, a similar form of pain. I am well pleased with the remedy in a fair case.

Dr. G. D. Bennett, of Newton, Kansas, reports:

CASE X.—In regard to my patient treated with theine, I would say that Mr. G. fell from his wagon in the early part of November, 1885, and dislocated his shoulder downward into the axilla, and also injured the brachial plexus of nerves in the fall. The latter was followed by some neuralgic pain, which grew to such severity that one-half grain doses of morphia gave him but partial relief. Some six weeks after receiving the injury and when the pain in the shoulder was steadily growing worse, I commenced using theine hypodermatically by injecting one-third of a grain in the morning. The result of the first injection was some redness and pain at seat of injection, and in fifteen minutes numbness of finger-ends appeared, which gave way in half an hour to a condition explained by the patient as "arm being asleep." In a short time, probably forty minutes, pain in shoulder all gone. Next day visited patient and found return of pain, but not so severe as before, and again injected one-third of a grain, which relieved him to such an extent that he had a comfortable day and an easy night. This treatment for the next twenty days was combined with tonics, at the end of which time my patient had complete use of his arm and no pain. I feel that we now have a remedy which is certainly far superior, at least was in this case, to morphine and cocaine, for they both had been tried thoroughly.

Dr. C. H. Castle, in the article to which reference has already been made, relates four cases of pain treated with theine as follows:

Case XI.—The patient was the subject of those ill-defined, dragging musculo-rheumatic pains of the extremities, found not infrequently in such a damp climate as is ours. About one year ago he had obtained marked relief from an electric bath, taken in Chicago, and at the time I encountered him he was on a search for a similar electropathic institution in Cincinnati. I persuaded him to allow me to use one-sixth of a grain of theine upon him hypodermatically. The pain was severe in the anterior muscular masses of both thighs and in the calves of both legs. I made my injection slightly below the popliteal space and toward the inner side of the leg. The effect was striking, and almost instantaneous. The point of injection, though angry looking, was quite anæsthetic, and as little tinglings shot down the leg toward the foot, the pain vanished and the analgesia of the injected leg was in marked contrast to the pain in the thigh of the same extremity, and the thigh and leg of the opposite extremity. This beneficial effect was maintained for four days, at which time I last saw the patient, and I exceedingly regret that we have neither of us since had the time or opportunity to relieve the still suffering fragments of his body of their aches.

The other case I will give from the notes taken by the attendant physician:

Case XII.—Myalgia of the deltoid muscle. Pain severe. Had had one-quarter grain of morphia every two hours the night before without bringing sleep. Could not raise arm.

9 p. m. One-sixth grain of theine subcutaneously about the anterior margin of the trapezius, just above the shoulder. Pulse 102.

9.04. Very little pain, unless she moved her arm. Still marked tenderness on pressure.

9.05. Pulse 96. Diminished sensibility about seat of injection.

9.10. Seat of pain seemed to be only in lower half of its former area.

9.13. Numb feeling in arm as far down as elbow. Very little pain on moving arm.

9.15. Can now bear pressure over former exquisitely tender spot.

9.20. Pulse 96. Only tenderness is now over acromion. No pain at all on pressure over anterior part of arm.

9.30. Numbness not so marked. Says if only had as much pain as she now has she could easily bear it.

9.37. Pain now only about insertion.

9.40. Numbness in arm diminishing, but pain not increasing. Says she could go to sleep.

11.00. Pain has returned, but in a much less degree. Needs no morphia.

Since then has not complained.

CASE XIII.—Neuralgia of the sciatic nerve. Chloroform, ether, etc., had failed as curative remedies, or even to relieve pain. Temporary relief was being sought with morphia.

4.05 P.M. Injected one-third grain of theine over the course of the sciatic nerve near it emergence from the pelvis.

4.10. Feels easier below the seat of injection. Still tenderness on pressure over the nerve.

4.20. Still somewhat better.

4.25. Jerking sensation when walking, no pain while lying in bed.

4.35. Tingling sensation in heel, walks much better.

5.00. Numbness from knee down to foot; warm flashes.

5.30. Same feeling as at 5 P.M., with pain in hip.

6.00. Pain not so severe in hip; numbness from knee down.

6.30. Complains of foot feeling very cold. It communicates this sensation to the head.

7.00. No pain, but still a sensation of cold. Hot sand bags applied.

Next morning pain had returned.

CASE XIV.—In this case there was absolutely no benefit derived whatever. The pain was not at all relieved.

It would be highly satisfactory if the nature of this last case of pain had been given by the author. In the absence of these important data, it is, of course, useless to speculate on the probable cause of its failure to act.

Dr. Washington H. Baker, of Philadelphia, reports the following cases:

CASE XV.—L. N. W., aged thirty-six, consulted me December 13, 1885, on account of a pain in his left hip, from which he had suffered for eight months. Never had any disease of the genitalia. Has not had chills and fever, nor rheumatism. Had an attack of typhoid fever thirteen years ago, and been well ever since until the present trouble appeared. The greatest pain complained of is in the left hip a little above the trochanter major, the pain also radiates around in front and down back of thigh. The pain does not extend below the knee at present. Some time ago the pain extended to ankle. Tongue clean. Bowels regular. Appetite good. Is compelled to use a cane and limps when walking.

Dec. 16. Gave a subcutaneous injection of twenty minims of a two per cent. solution of theine. He felt better at once and could stand straighter. In a few minutes had a tingling sensation in thigh. Can walk more erect and with less pain. The thigh feels numb when touched, the leg less so. Half an hour after the injection the thigh still felt numb, but he could not walk quite so well. Was given five grains of iodide of potassium thrice daily. The injection was given about three inches above the seat of pain.

17*th*. Injected twenty minims of theine. Helped him some, but not a great deal. Applied a thapsia plaster to thigh.

18*th*. Tells me he had less pain since yesterday, than since he had the disease. Injected twenty-five minims of theine.

19*th*. Walked down to office this morning, a distance of twenty squares. Is feeling much better. Injected thirty minims.

20*th*. Feels about the same as yesterday. Little or no pain. Injected thirty minims.

21*st*. Feels better than he has yet, less pain than yesterday. Injected twenty-five minims.

24*th*. Yesterday morning the pain returned in the limb. Last night it was just as bad as ever. Injected twenty minims, and after three minutes the pain began to grow less.

25*th*. Still considerable pain. Injected twenty-five minims.

26*th*. Had severe pain last night. Injected twenty-five minims.

28th. Still considerable pain. I gave a pill of sulphate of cinchona, ext. nux vomica, ext. belladonna, and pil. ferri carb.

Jan. 6. Feels better. Injected twenty-five minims of theine.

30th. Walked into office to-day, erect, very little pain complained of, and a slight limp. Considers himself cured.

CASE XVI.—J. B. came to me in December, 1885, complaining of a severe pain in the lumbar region which almost incapacitated him for work. Injected twenty-five minims of a two per cent. solution of theine in lumbar region above the painful part. He was relieved at once and has not been troubled with it since.

CASE XVII.—J. R. W., aged thirty years, brakeman, suffered from a pain in the lumbar region which troubled him particularly when he bent over to wash his hands. I injected twelve minims of a two per cent. solution of theine under the skin above the painful spot with immediate and entire relief of pain. It was really ludicrous to see the astonished expression on his countenance and the contortions he went through to assure himself that he was in truth relieved. I also gave him two and a half grains of iodide of potash thrice daily.

Dec. 22. Injected twenty-five minims of thiene, as there was a suspicion of pain in the back. This visit was one week after the first.

Jan. 12, 1886, reports no trouble with his back.

CASE XVIII.—March 8, 1886, a gentleman, fifty-eight years old, told me that he had been suffering for several weeks with a pain in the left shoulder. He supposed it was rheumatism. He had been kept awake the greater part of the last three nights on account of the pain. The pain was getting worse from day to day. I injected twenty-five minims of a two per cent. solution into the shoulder with immediate relief. That night he slept comfortably, and has not had any pain in shoulder up to date (March 20, 1886). After the injection a burning, tingling sensation was complained of at site of injection. The theine had no appreciable effect on the mental faculties.

Case XIX.—March 20, 1886, a patient complained greatly of a bunion on right foot. The pressure of the shoe in walking caused considerable suffering and a slight limp. Fifteen minims of a two per cent. solution of theine were injected about three inches above inner malleolus. Four minutes after injection the bunion could be roughly stroked and pinched without discomfort. The shoe was put on, and walking caused no pain whatever.

Dr. J. H. S. writes from Bolivar, N. Y., under date of December 21, 1885, about his own case as follows:

Case XX.—" Dear Sir: I have just this day read your article on the 'Therapeutic Action of Theine,' and would say that my own case is almost a *fac simile* of your case, Mrs. A. A. (which was a case of sciatica). I have tried every remedy I could think of, but have found nothing to give relief but injections of morphine. I had to give up the morphine on account of the constitutional effect which it brought on. My trouble was caused by being thrown from my cutter about a year ago, but had no severe pain until last August, when I became a subject of severe blood-poisoning contracted during an operation, and from which time the pain in my limb began to be terrible. I went to the springs and continued treatment for the poison. I feel so badly now that I find it impossible to write you in full. If you will be kind enough to send me some theine, with full instructions how to use it, I will test it and have my case carefully reported to you. I would further say that I returned from the springs about three weeks ago, but received no benefit. I am now and have been in bed for the past ten days; and I only hope this new remedy will give me some relief from the most terrible pain possible to endure."

Under date of March 22, 1886, Dr. S. states:

" In relation to the theine, I will say that it has done its work for me. The first injection was made on December 25th, one-third of a grain in the morning, and one-third of a grain more in the evening. Then I followed this up with daily two-third grain doses for six

days, then two doses of the same amount for the next three days, then two doses in five days, and the last one in six days afterward. After this I was free from pain. My leg, which was three inches smaller than the opposite one, is now about its natural size. I am a thousand times your debtor for the theine you sent me, for I have full confidence that it gave me relief, and, so far as my sciatica is concerned, it made a cure without any bad effects."

Case XXI.—I have tried theine in two other cases, but was not able to watch the effects as I would have done had I been in condition, but can say one was a strong man with rheumatism of the left arm. I gave him two injections of two-thirds grain each, which relieved his pain.

Case XXII.—The other was a man of about fifty years old, with sciatica, which had troubled him for years. I gave him two-third grain doses once a day for three days, which gave him incomplete relief. I then gave him a grain a day for two days, and this gave him entire relief. I will say, however, that this case I have not seen since I gave him the last injection.

Since the appearance of my last article on this subject in *The Medical News*, I have treated quite a number of cases of pain with theine, of some of which I beg leave to give a condensed report in this connection.

Case XXIII.—Mrs. B., aged thirty, was entirely relieved of intercostal neuralgia, accompanied by a peculiar constant burning pain in the left interscapular space by one injection, which was given over painful spot along left side of spine.

Case XXIV.—Dec. 5, 1885. V., Chronic rheumatic pain of left shoulder-joint and whole arm. Pain very severe. No relief rendered previously by salicylates, muriate of ammonia, iron, quinia, or iodide of potash. One-half grain of theine relieved the pain at once and made him feel comfortable.

Case XXV.—Dec. 17, 1885. Mrs. W., aged fifty, suffered from cervico-brachial neuralgia on right side since the previous August. Two injections of half a grain each, a week apart, were sufficient to relieve the pain.

Case XXVI.—Dec. 29, 1885. M. C., aged forty-seven. Pain in and inability to move left arm for three weeks. Unable to lie on left side. Could not dress himself without aid. Pressure along the region of the left brachial plexus greatly intensified the pain along the shoulder and the arm. Injected half a grain over seat of pain along left side of spine, and in less than five minutes he was able to lift his arm to his head, to swing it to his back, and, very much to his surprise, to dress himself—something which he had not been able to do for three weeks. He said he was entirely relieved. Have not seen him since.

Case XXVII.—January 7, 1886. Case of double sciatica of fourteen years' duration. Theine injections relieve the pain, but are powerless to cure the degeneration which has probably taken place in the nerves.

Case XXVIII.—January 12, 1886. Sciatica in left leg relieved by two injections.

Case XXIX.—January 12, 1886. K., aged forty-five. Burning pain in right interscapular region, which was entirely relieved by one injection over the seat of pain.

Case XXX.—February 9, 1886. He was cured of a brachial neuralgia by one injection.

Case XXXI.—February 18, 1886. Pain in the lumbo-sacral region; also complains of pain in left brachial plexus. Gave him two injections—one over the former and the other over the latter seat of pain, and he has not complained since.

Case XXXII.—December 16, 1885. Severe case of sciatica, of two weeks' duration, was relieved by seven injections.

Case XXXIII.—February 18, 1886. Intercostal pain aggravated by pressure along left side of spine in upper dorsal region. One injection over latter place cured him.

Case XXXIV.—February 3, 1886. W. This was a most aggravated case of neuralgia of both lumbo-sacral and left cervico-brachial plexuses, associated with irritability of the spine along its whole course. The pain had been coming on for two months or more. It was intermittent, very intense, and accompanied by muscular contractions of both legs below the knee and left forearm. It was impossible for patient to stand erect when he suffered from the pain—in fact, he was confined to his bed most of the time. Four injections, of one-sixth grain each, in the evening, made him comfortable during the night and next day until evening, which, with other remedies, like iodide of potassium, iron, muriate of ammonia. and quinia, have been kept up for nearly three weeks; and now the patient is in a good condition, able to walk twenty-five squares in one stretch, and only requires an occasional injection. The injections were all made on each side of the spine, in the interscapular and lumbo-sacral region, and never failed to give him instantaneous relief, no matter how bad the pain was at the time.

Case XXXV.—M., æt. twenty. Carbuncle in posterior part of right thigh, from the pain of which she suffered intensely. Injected one-third grain of theine about three or four inches above the seat of pain, and she became easy from that time on until it was lanced. She is accustomed to the pain of carbuncles, and says that the injection gave her immediate relief.

I might multiply these cases, for I have treated a number more in the same way, but I presume that what has already been given, especially the history of those cases given before my own, is sufficient to show that theine is of considerable clinical value. In addition to the theine treatment, I will say, that all my aggravated neuralgiæ had received iron, quinia, ammonia, iodide of potash, and arsenic. For it must be remembered that

the function of theine is solely that of pain-relieving, the intimate nature of which is not understood; and while it performs this office to perfect satisfaction, it also fails to give permanent relief to obstinate and protracted cases of pain unless it is combined with remedies which assist in changing the nutritive state of the affected nerves.

The question now arises, Is theine an analgesic in all kinds of pain? This is best answered by referring to its physiological action. I think my experiments on frogs demonstrate that while it reduces sensation when locally applied, its chief characteristic action begins at the spinal cord and spreads thence to the periphery of the nerves. Hence it is indicated in all pain of central, especially of spinal origin, as in that of the different forms of neuralgia, of neurasthenia, of locomotor ataxia, of ischæmia of the spinal cord, and of spinal irritation. I have also seen it act very well in lumbago. But pain depending upon peripheral irritation, such as that caused by an injury to the surface of the body; or that caused by a carious tooth, etc., is more successfully treated by the surface or subcutaneous application of cocaine.

The action of cocaine is in many respects the reverse of that of theine. It does not, like theine, produce anæsthesia of the skin or mucous membrane by affecting the trunk of the nerve and thence all its terminal filaments, but by coming in direct contact with the endings of these filaments. It is for this reason that cocaine has failed as an analgesic in neuralgia. I do not wish to assert that cocaine is incapable of affecting the nerve trunk in the same way as theine does if it were injected deep enough to bring it in direct contact and then con-

fined there, or, at least, below the seat of injection, by constricting the circulation of the limb, as has lately been suggested by Dr. Corning, of New York; but when cocaine is injected subcutaneously in the ordinary way its transient anæsthesia will be limited chiefly to a small circumscribed area at the seat of injection, and will not affect the nerve trunk beneath.

This distinction is important, for it shows that theine, while it is a local anæsthetic in one sense, is not so in the same sense as is cocaine. The former when injected subcutaneously renders a whole or a part of a limb numb, but it is powerless to reduce sensibility to the same degree as cocaine does when applied locally. The fact that theine chiefly acts from the centre to the periphery, makes it very important, in a practical point of view, that it be introduced above the seat of pain, or, at least, over the central origin of pain, in order to secure its full analgesic influence.

The localized action of theine brings out one of the advantages it posseses over morphine and other analgesics. It is pretty well established that morphine, narceine, and other agents of the same class, bring about analgesia only by chiefly intoxicating the brain and higher nerve centres, and leaving the peripheral nerves more or less uninfluenced. This undesirable feature of morphine action is absent in theine, for it seems to leave the brain entirely undisturbed, at least so far as any narcotic influence is concerned, unless given in very large doses.

Theine, as a rule, is surprisingly prompt in exerting its analgesic action. I have on a number of occasions witnessed patients who had too much pain to move the arm, or too stiff to stand erect, swing their arms and

straighten their bodies in less than five minutes after its introduction. It is not always so prompt, but this may be due to the smallness of the dose. I have seen cases where no benefit had been derived from one-third of a grain, which, on the dose being doubled, improved at once. Therefore, when a single dose fails to act favorably, it is always advisable to increase, even if this has to be done to a large extent. I have repeatedly injected two grains, not in a single, but in four separate injections, in as many different localities along the back, within the space of five minutes, and found nothing but the best results.

Another feature which recommends theine to favor is its prolonged influence. After pain has once been relieved by it, it is very rare to find the same returning inside of ten or twelve hours, and oftener not until in twenty-four hours, and then, in all probability, not in its original intensity. I am referring now to obstinate cases of pain. Where the pain is more unstable, it is not a seldom occurrence to find that one or two injections relieve it permanently.

In order to obtain the full effect of theine, it is not necessary to inject it deeper than immediately beneath the skin. Of deep injections I am not able to speak intelligently, always having been satisfied with the results derived from superficial injections. Dr. Castle, in the article referred to, relates his own personal experience with one deep injection, in which the drug certainly exerted a marked influence on the brain, owing, probably, as has already been stated, to the fact that the whole dose received direct entrance into the circulation, and was at once carried to the centres of innervation.

No prolonged irritation and no inflammation are pro-

duced at the seat of injection. It gives rise to considerable pain or burning at first, but this disappears in the course of a very few minutes, and is replaced by a marked area of anæsthesia. Most of the burning is undoubtedly due to the comparatively large amount of water which is necessary to dissolve a drug of a solubility as low as that of theine in cold water—which is one in fifty parts. This can be overcome by dissolving the theine in warm water, in which it is very soluble, even at the normal temperature of the body. In this way a whole grain can be dissolved in a very few minims of water.

In regard to the employment of a particular kind of theine, I would say that it is tolerably certain that everything which falls within the commercial description of both drugs is principally made up of theine: hence very good clinical results are obtained from that which is found in the market—especially that of Merck, which I have principally used—although it is very obvious that, if my experimental deductions in regard to the differential action of theine and caffeine are correct, much better results are obtained from theine when unalloyed with caffeine.

Now, gentlemen, I hope that I have not wearied you unnecessarily in bringing this subject before you at some length, and in conclusion beg to say that all the practical value which the contents of this paper may have, emanates from the pharmacological laboratory, for it was through its methods alone that the first clew to the analgesic action of theine was discovered. I therefore trust that you will receive it as a gift from the experimental department of medicine to and in the interest of that science which we all delight to honor and to encourage, viz., internal therapeutics.

[After the reading of the preceding paper :—]

Dr. Theodore G. Wormley said: I would simply remark that we should hesitate very much in drawing contrasts between these two alkaloids, theine and caffeine. They have been studied very thoroughly, and proved to be identical in composition and in chemical properties. We unquestionably have here very different results from these preparations. Where the same alkaloid exists in two different substances, it is sometimes very difficult to extract it perfectly pure. We have a good example of this in daturine and hyoscyamine. They are identical, but it is difficult to obtain them pure, and they generally produce very different physiological effects. We should, therefore, be careful in drawing conclusions from such experiments, unless the alkaloids were examined chemically, and an ultimate analysis made of their chemical properties. Otherwise, we might be led into error. We have another illustration in aconitine. I have seen 0.003 gr. of crystallized aconitine kill a white mouse with characteristic symptoms, while 0.03 gr. of Merck's aconitine produced no such effects.

Dr. Mays said: I feel that a very important question is at stake. It is the question, whether two preparations chemically identical must produce the same physiological effects. I could not account for the differences which I found between theine and caffeine, unless, as has been said, they are differences in degree; that is, that the same action is characteristic of both, but present in a different degree in both. I can only explain it that way, or on the view that either theine or caffeine is a compound product. We know that caffeine and theine can be transformed into caffeidine, and certain German investigators have found this to be analogous in its action to caffeine and theine.

I do not think it entirely necessary to suppose that two bodies that are chemically identical or isomeric, must have the same physiological action. We have an illustration of this in daturine and atropine, which are supposed to be chemically identical, or nearly so, and yet they have very different physiological effects. Lately, in the *Journal of Physiology*, I saw that Drs. Moorehouse and Ringer, in investigating the common daffodil, found that the alkaloid obtained from the bud before flowering, although identical in chemical composition, was quite different in its effects from that obtained after flowering had ceased. One produced salivation, while the other dried the secretions of the

mouth and skin, and the differences between the two were shown in other respects. From the latest physiological investigations, we do not need to conclude that because two bodies are chemically alike, they are identical in their physiological effects. I would make no rash assertions on this point, but should like to hear the results of further investigation.

Dr. WORMLEY: I do not mean to say that identity of composition implies identity of action. We have any number of isomeric bodies. What I mean to say is that, in the separation of these bodies, we have other bodies present in the plant which may be carried over with the alkaloid. We meet with certain bodies in the extraction of the alkaloid from coffee, and we meet with certain other bodies in the extraction of the alkaloid from tea, and it is likely that some of these may be carried over with the ultimate product. It is not simply isomerism, but the obtaining of these substances in an absolutely chemically pure state.

Dr. FRANK WOODBURY said: I should like to offer the following from Dr. E. R. Squibb, of Brooklyn, in connection with the paper of Dr. Mays. Dr. Squibb says:

"1. There is no such substance as theine as distinct from caffeine; the same substance, chemically, being obtained from tea and coffee. Whether it preëxists in both, or either, I doubt.

"2. Tea and coffee may have, as commonly believed, different physiological effects, but the substance obtained from them has the same effect.

"3. Damaged teas and coffees are commonly both sold to make caffeine from, but it is probable that most of the caffeine is of late made from tea."

Dr. MAYS: I do not know that Dr. Squibb brings out anything new, except the somewhat novel view that these alkaloids may not preexist in tea and coffee.

THE OPHTHALMOSCOPE FOR THE GENERAL PRACTITIONER.

By
EDWARD JACKSON, M.D.,
CHIEF OF THE EYE CLINIC IN THE PHILADELPHIA POLYCLINIC.

[Read April 7, 1886.]

The want of a form of ophthalmoscope specially suited to the needs of physicians who do not make the objective measurement of refraction an important part of their professional work, has been an obstacle to the more general practice of ophthalmoscopy. It is not my purpose this evening to announce the invention of an instrument to meet this want, but briefly to discuss and to incite to discussion of what should be the characteristics of such an instrument, that there may be better appreciation of a standard to guide practitioners in choosing among instruments now extant, and inventors among modifications that are possible in the future.

"The physician," says Hughlings Jackson, "is quite as much indebted to Helmholtz as the ophthalmologist." There is exaggeration in the statement; but under the exaggeration lies this truth: the physician's obligation to the inventor of the ophthalmoscope is great. How great we do not yet know; but some appreciation of it will be gained by a careful examination of such treatises as Gowers' *Medical Ophthalmoscopy*, or Dr. Wm. F.

Norris' very condensed article on "Medical Ophthalmology," in Pepper's *System of Medicine.* The direct value of the ophthalmoscope in the consulting-room has been strenuously insisted on, and often illustrated. I will not dwell on it now; only let me remind you of the indirect importance of its use, as a means of training eye and brain for diagnosis in general. All schemes of education recognize the high value of some studies as means of mental development and discipline. For training the powers of observation and reasoning that must ever characterize the acute diagnostician, I think the practice of ophthalmoscopy is unequalled. In it the student is incited to the certainty and exactness of microscopical research, yet must ever consider the personal diversities of patients. Then there is the wonderfully impressive character of the phenomena studied with the ophthalmoscope. In the words of Dr. Loring: "With it, it is like walking into Nature's laboratory and 'seeing the Infinite in action,' since by its means we are enabled to look upon the only nerve in the whole body that can ever lie open to our inspection under physiological conditions, and to follow in a transparent membrane an isolated circulation from its entrance through the arteries to its exit through the veins. We are further enabled to study daily, or even hourly, morbid processes in each and every phase." These morbid processes are not only important in themselves, they have their close analogues in every organ, and in almost every tissue of the body. The value of this close study of pathology, of gaining insight, of getting into sympathy by close contact with natural processes, healthy and morbid, can hardly be overestimated. Well might the great German pathologist exploit the ophthalmoscope,

and wish that the methods of ophthalmology might be adopted in other branches of medicine. I submit the proposition that the more general use of the ophthalmoscope would tend toward such a desirable advance.

The instrument suited for this general use must have *a few well-chosen lenses:* say, convex, 1, 2, 3, and 5 dioptrics; concave, 1, 2, 4, 6, and 10 dioptrics. Only these few are necessary. Higher degrees of hypermetropia are not common, and in such cases the fundus is readily examined either by using the accommodation, or by withdrawing a little from the eye under observation; while higher degrees of myopia are best studied by the indirect method. Unnecessary lenses, besides adding to the weight, complexity, and cost of the instrument, constitute a real and serious obstacle to its use. Often the ophthalmoscopist sees by glimpses. The position of the light, the ophthalmoscope, and the observer's eye, must be adjusted to the patient's position before the first glimpse can be obtained, and whenever the patient moves the process of adjustment must be repeated. If the process of adjustment be prolonged by the necessity of running through a long series of lenses to select the proper one, it may be scarcely completed and the first glimpse obtained, before it has to be again repeated.

Now by long and frequent practice it becomes possible to make these adjustments very rapidly and automatically, and also to observe at a glance the condition of the parts under inspection, so that the ophthalmoscopist in constant practice finds the use of his instrument easy and satisfactory even though it be of a somewhat complicated pattern. But the student or practitioner who has not this perfect familiarity with

the process, finds every additional difficulty of adjustment a very serious hindrance.

For this same reason it is highly important that the *lenses be most conveniently arranged*, so that the whole series is easily and quickly available when once the illumination of the fundus has been secured. They should be mounted in the Rekoss' disk used in most refraction ophthalmoscopes, or in slides such as I have proposed for the purpose (*Trans. Amer. Ophthal. Society*, 1885, p. 111). The choice and the arrangement of the lenses are the especially weak points in the Liebreich ophthalmoscope, with which, probably, more medical men have essayed the practice of ophthalmoscopy than with any other one form of instrument. It may not be without interest to mention that one of the first attempts to improve on the Liebreich was made in this city over twenty years ago. At the instance of Dr. Charles H. Thomas, through whose courtesy I am able to show this instrument, Queen & Co. modified the common Liebreich ophthalmoscope by mounting the mirror in a hard-rubber handle, and placing the lenses in a revolving disk behind the mirror. This is, of course, practically the Rekoss' disk, but in this case the name is inappropriate, for Dr. Thomas got the idea not from the work of the Dutch optician who seems to have immortalized himself by his addition to the ophthalmoscope, but from the revolving diaphragm plate of his own microscope.

Another requirement is: *the aperture in the mirror should be small*, to lessen the circles of diffusion when ametropia cannot be exactly corrected, and especially to secure a sufficient illumination of the fundus through the undilated pupil. Our popular American ophthalmoscopes have a mirror aperture of from three and a

half to four millimetres. This is none too large in an instrument used for the accurate measurement of the errors of refraction, where it is desirable to emphasize rather than suppress the circles of diffusion. On the other hand, many European modifiers of the ophthalmoscope have chosen smaller apertures, thereby reducing the value of their instruments as refraction ophthalmoscopes, but making it much easier to study the details of the fundus with them.

The inclined, or tilting, mirror should be used. Practical ophthalmoscopists find it more convenient and satisfactory, and to those less accustomed to the use of the instrument it is of much greater importance; less, perhaps, for its theoretical advantage of having the plane of the correcting lens perpendicular to the axis of vision, than for the greater ease in keeping the fundus properly illuminated. As a rule, the tilting mirror has, heretofore, been confined to the more costly and elaborate refraction ophthalmoscopes. There is one exception to this rule that merits notice. In 1882, Dr. Baumeister described a form of ophthalmoscope presenting no features of especial importance to the ophthalmic specialist, and hence, as yet, little known outside of Germany. Through the aid of Queen & Co., who have imported it, I am able to show a specimen of this form of instrument. Its special advantage is, as you see, a mirror tilting either way to an angle of twenty-five or thirty degrees. The aperture of the mirror is but two and a half millimetres. It has also a good series of seven lenses arranged in a Rekoss' disk. Moreover, this instrument is sold for but little more than the old Liebreich ophthalmoscope. This matter of cheapness is not unimportant for an instrument that is to aid in generalizing the practice of ophthalmo-

scopy. Despite importance of thorough equipment for professional work, the cost of a good instrument keeps many from making its practical acquaintance. Its deterrent power is especially felt by the recent graduate, just when the hospital and dispensary opportunities, and comparative leisure in private practice, offer him the best and perhaps the only chance for gaining facility in the use of the instrument; and just when the insight it gives into vital processes would be of the greatest value.

But, as in Baumeister's ophthalmoscope, *cheapness should be secured by simplicity, never by poor workmanship.* In no instrument of diagnosis, not excepting the microscope, is perfect workmanship more important. There is none in the use of which mechanical imperfections are so serious a hindrance. And good construction includes more than the general strength and durability of the instrument. It includes thin lenses, freedom from reflections at the sight-hole, a spring catch to centre accurately the lens in use, mounting the lenses so that they can be readily cleaned and kept clean, and other details which might seem unimportant, but which all help to determine the ease and satisfaction with which a given instrument may be used.

To recapitulate, the physician who does not undertake the correction of exceptional anomalies of refraction, needs an ophthalmoscope simply but well made, with a few well-chosen lenses, conveniently arranged, and a tilting mirror with a small central aperture or sight-hole. When the use of such an instrument becomes more or less familiar to well-trained physicians engaged in all branches of medical practice, we shall be able to appreciate better the debt of the physician to the inventor of the ophthalmoscope.

REMARKS ON LANOLIN.

By

THOMAS G. MORTON, M.D.

[Read April 7, 1886.]

Although the members may be familiar with the new substance, lanolin, introduced by Liebreich, I thought it might be interesting to exhibit a few specimens of this preparation from the wool of sheep. Those who have used it, have been much pleased with its employment in the preparation of ointments for external application. It can be readily rubbed into the skin and produces no irritation, and, therefore, would probably be valuable in massage. It is probable that substances combined with lanolin will be more readily absorbed than if prepared with other bases. One of the remarkable properties of lanolin is that it can be readily combined with water. I show you a specimen containing sixty per cent. of water.

I quote the following from the *American Journal of Pharmacy*, February, 1886:

"Under the name of lanolin, Prof. Liebreich introduced the fat obtained from sheep-wool. One of its properties is to take up more than its own weight of water. Unna states that cooling ointments should contain large quantities of water. Dieterich, with a view to determine the quantity of water taken up by different salve bases,

experimented with twenty-one different bases at a temperature of 15° C., taking for each experiment 100 parts of the base. His results were as follows: Cosmoline took up 4 parts of water; lard, 15; benzoinated lard, 17; and lanolin, 105.

"Lanolin, as it now appears in the market, is a perfectly neutral base, and hence is not apt to decompose any medicament which might be added. Lasser experimented with it on 400 patients, and states that lanolin is readily absorbed by the skin, does not produce any irritation, and permeates the lower layers. Bachmann rubbed a ten per cent. iodide of potassium ointment, made with lanolin, into the skin for five minutes. After the lapse of half or three-quarters of an hour, iodine was detected in the urine. The elimination of the iodine by the urine continued for fourteen days after several applications of the ointment. Lanolin was used by the ancients, and is mentioned by Ovid, Heredot, Plinius, and Aristophanes."

I have received the following from Mr. McKelway, the druggist, who made the preparations shown to-night:

"All the published articles I have seen are very chary of any description of the detail of its manufacture. Liebreich patented his process in the United States in January, 1883, and the process patented by him is concisely stated as follows:

"He takes the suds from the washing of wool in the mills, submits it to the action of a centrifugal machine which separates the soapy, oily suds from the dirt associated therewith, decomposes the suds by an acid, whereby the acid and the saponifying alkali unite and the saponified wool-fat is separated, combined with about 100 per cent. of water; this is then thoroughly washed with cold water, then heated so as to separate the water and the wool-fat, and again combined with a definite proportion of water, and lanolin is the result.

"Or, he treats wool with alkaline water, producing his suds in that way, and then proceeding as I have already outlined.

"A much quicker and less complex way of making the article is to treat the wool directly with petroleum benzine; distil off the benzine, and the wool-fat remains; combine this with a proper pro-

portion of water, and lanolin results. In his patent specifications, Liebreich speaks of this process, but says it is objectionable because of the difficulty of getting rid of the benzine odor. In the exceedingly short time I have had to prepare what I have prepared, this shorter process was the only one that I could use, and the samples I present were so made.

"Wm. M. Coates, Esq., wool merchant, tells me that Merino wool, clipped without the sheep being washed, contains the enormous proportion of sixty per cent. of this fat. I enclose samples of the wool. If you will twist very tightly together a number of filaments of it, you will be able to collect the oil on your finger-nail. I also enclose some of the same fleece from which most, if not all, of the oil has been extracted with benzine.

"I send, also, a sample of the oil, labelled 'wool-fat;' and also a jar of lanolin, made by me by mixing this same wool-fat, 100 parts, with water, 30 parts (it is so labelled); and also a sample of lanolin imported for me by Messrs. Lehn & Fink, of New York.

"The cost price of the imported lanolin is now, in large quantities, $1.00 per pound; a very little while ago it was $2.00 per pound. I believe it will ultimately be about as cheap as lard.

"When it is considered that all wool averages about forty-five per cent. of its weight of this fat, that it has all to be removed before the wool can be manufactured into fabrics, that up to this time it has had no value, that the process for its extraction is not a very expensive one, and that lanolin is thirty per cent. water, I think you will be convinced that lanolin will certainly be as cheap as lard as soon as competition in methods of extraction and in supplying have an opportunity to affect its price.

"Billions of pounds of this wool-fat have hitherto been thrown away every year.

"If lanolin is what it is claimed to be therapeutically, a new and immense industry opens to the world."

The *Therapeutic Gazette* of March 15, 1886, speaks as follows of the practical uses of lanolin:

"From his investigations of the composition of the cholesterine fats found in keratinous tissue, Dr. Oscar Liebreich (*British Med.*

Journ., Feb. 13, 1886) conjectures that the absorption into the skin would be best in the case of those fats which have their origin in the keratin-bearing substances, as hair, epidermis, etc. The old theory that the skin was only oiled from glandular secretion, did not harmonize with these researches; and lanolin, upon his suggestion, is now being tested as to its efficacy in therapeutics as a new basis for salves and ointments. It is of importance to add other ingredients to make it more pliant, as it is a too sticky mass in itself to be employed alone; and from many trials which Dr. Liebreich has made with different substances—as vaseline, paraffine ointment, glycerine, oils, and fat—for this purpose, he has found the latter by far the best, as the others generally interfere with the absorbing qualities of lanolin. On exposure, the upper surface of lanolin, and all lanolin salves and ointments, becomes darkened, due to the evaporation of water and not to its decomposition."

Speaking with Dr. Leidy with reference to lanolin, he said that the skin of the sheep has an enormous number of sebaceous glands, and that he was not surprised at the amount of fat taken from the wool. The usual way of making mercurial ointment—by rubbing up the mercury with mutton suet—is a rather troublesome and tedious proceeding. If lanolin is used, it can be made as perfectly in thirty or forty minutes as by the longer process. It is said that the 0.001 per cent. of bichloride of mercury with lanolin rubbed upon the skin, can be tasted in the mouth in thirty minutes.

[After the reading of the preceding paper :—]

Dr. KEEN remarked : I have used lanolin in several instances during the last week or ten days. One case was that of a child, eight years of age, with an enlarged gland under the jaw, the size of an English walnut. I prescribed iodine, two grains to the drachm of lanolin. I saw the child a week ago, and the gland had almost entirely disappeared. This was a more rapid disappearance than I have ever seen from other iodine applications. This is the only case in which I have had any results, although I have used it in a number of instances, and like it very well.

CONSTIPATION.

BY

ARTHUR V. MEIGS, M.D.,

PHYSICIAN TO THE PENNSYLVANIA AND TO THE CHILDREN'S HOSPITALS.

[Read May 5, 1886.]

THE title I have selected is one which includes a much wider field than I shall at all be able or intend to cover within the narrow scope of my paper. My object is merely to give a brief account of such cases as have come under my notice, and to draw what inferences I am able with regard to the lessons to be learned from their study.

CASE I.—A woman now eighty-three years of age has been constipated for many years past, and is in the habit of taking almost daily a pill which consists of ext. belladonnæ gr. $\frac{1}{12}$, ext. nucis vomic. gr. $\frac{1}{4}$, aloes gr. $\frac{1}{2}$, and rhubarb gr. j. She has sphincterismus and some slight narrowing of the anal orifice, with hypertrophy of the sphincter and slight retraction upward of the anus. There is nothing this woman dreads more than constipation, for some years ago she had an attack, when the rectum became so full that the contents had to be removed mechanically. During the past four or five years she has been subject to attacks of chills, followed by nausea and vomiting. These attacks occurred every six or eight weeks, or sometimes at longer intervals, and prostrated her much, confining her to bed, usually for two or three days. They were attributed, by one of the physicians who saw her, to malaria and treated accord-

ingly. Last winter, after long puzzling (for I have known this patient professionally for eleven years) as to the possible causes of these attacks, it struck me that they might be due to digestive disturbance, as they were so invariably accompanied by nausea and vomiting, and because when she had an attack she would invariably complain that her bowels were not properly opened, and this although there had been a movement every day. I invariably gave some aperient medicine, usually merely an increased number of the pills that have been already mentioned. In the early winter of 1885 she had one of these attacks, and after four or five days when she had taken the aperient pills until she had three or four movements daily, which her maid declared were large and loose, she still complained so bitterly of a sensation as if there was something in the rectum, that I made a digital examination. My surprise was great to find that the rectum was full of feces of the consistence of soft brick clay which it was quite impossible to remove with the finger, as it merely passed through the mass in any direction. It was equally plain that enemata would not effect the purpose, for she had had as many as four or five on each of the past two or three days, and although each one would bring away some little feces the discomfort still continued as great as ever. I accordingly directed that she should be given one of the pills four times a day until she had five or six movements each day, and they had ceased to be at all pasty and had become watery. This entirely removed the trouble and the amount of the movements was very large. This convinced me that the cause of the attacks was a gradual accumulation of fecal matter in the intestine, probably in the sacculi, notwithstanding the fact that previously there had been large and natural movements almost every day; and, consequently, that every six or eight weeks a quantity of this material larger than her own unaided forces were competent to get rid of, descended into the rectum. Acting upon this belief, I directed that on two days of every second week she should take three of the pills, hoping thus to aid nature in getting rid of any accumulation that might take place. This plan being followed out, she had no more attacks for about six months, and the only apparent effect produced by the pills was that on the days they were taken there would be possibly two movements, although generally only one, and that these

movements were very large. During the summer she was in the country and consequently was attended by another physician. I was sent for at midsummer to find that she had been sick a week and was much prostrated, and had nausea and *constant diarrhœa.* Suspecting that the old trouble might be the cause of the attack I made a digital examination of the rectum, to find my suspicion correct, for it was again full of feces. After much difficulty, for it was hard to bring the physician in the country to see the necessity for giving large doses of aperient medicine to cure a diarrhœa, she was finally relieved. It was only, however, after I had removed what I could of the mass with my finger, had given numerous injections, and she had taken four times daily for four or five days a pill consisting of ext. belladonnæ gr. $\frac{1}{12}$, ext. nucis vomic. gr. $\frac{1}{4}$, ext. colocynth. co. gr. ij.

Case II.—In the autumn of 1881 I attended an unmarried woman of about sixty years of age, with a bad cold which finally took the form of a mild bronchitis, but did not confine her to bed. She was a tall, thin woman, and had all her life been frail and delicate. After about three weeks, when the bronchitis seemed nearly well, and I was beginning to think that in a few days more she would have entirely recovered, she suddenly and without apparent cause began to get weaker, would occasionally vomit, and lost all desire for food. Soon she was confined to bed, and as she was constipated I ordered a seidlitz powder, which had about the usual effect, producing one or two watery movements. This condition continued for a week or two, the woman becoming all the time weaker, vomiting once or twice a day, suffering with almost constant nausea, and having the bowels moved only when some medicine was given. The constipation was not at all a marked feature in the case, for a movement could always be provoked by giving a seidlitz powder or half an ounce of castor oil, which was given once or twice, and the movements induced by these medicines did not present any unusual features, being, as is generally the case, loose, and somewhat watery. Examination of the abdomen showed it to be flat, but as there seemed to be some slight doughy induration along the line of the colon a suspicion of the true cause of the symptoms at last struck me and I ordered a mild pill consisting of ext. belladonnæ gr. $\frac{1}{12}$, ext. nucis vomic. gr. $\frac{1}{4}$,

aloes gr. $\frac{1}{8}$, rhub. gr. $\frac{1}{4}$, to be taken three times a day. After she had taken this pill for six or seven days I was shown one morning four enormous passages. These had been passed within a short time of each other, and consisted entirely of formed matter which must have been between an inch and a half and two inches in diameter, and perhaps two or three feet in length. From this time her recovery was rapid and very soon was complete.

CASE III.—A woman about seventy-five years of age, who has been habitually constipated as long as she can recollect, and has for a long time had sphincterismus and consequent narrowing of the anal outlet, with some retraction upward of the anus, had in the winter of 1884–85 an attack of constipation for which she took a great deal of purgative medicine in the course of a week, without ridding herself of a sensation as though there was an irritating foreign body in the rectum. By the end of the week she had hemorrhoids, which were very tense and painful, and the offending body, she said, came frequently to the anus, but her utmost efforts failed to rid her of it. Even when she tried to remove it with the finger, it slipped back into the pouch of the rectum, eluding her grasp. Upon digital examination a mass was felt in the rectum, and by hooking the finger around it it was removed with some slight difficulty and with great suffering on the part of the patient. This mass was probably from an inch and a half to an inch and three-quarters in diameter and nearly circular, and, therefore, not so large but that it could easily have been extruded from an anus of natural size. It was so hard that no effort enabled me to break it with my finger, and was yellowish-white and incrusted with a chalky substance, so that in both appearance and consistence it much resembled a freshly passed gall-stone.

CASE IV.—A young woman of about twenty-two, took, at the recommendation of a friend, a tablespoonful of sulphur on two successive days for dysmenorrhœa, with which she suffered. I was sent for the next day, as she had diarrhœa which did not amend. From this time, for between two and three weeks, she had constant fever, the maximum temperature being 102° F., with some sallowness of

the skin, pain in the abdomen, with tenderness in the left iliac fossa and complete loss of appetite. The movements of the bowels were thin and watery, and the case presented many of the features of a mild attack of typhoid fever; but Dr. Da Costa, who saw the patient with me, and myself both agreed that it was not so. The case was in so many respects like typhoid fever that the differential diagnosis was very difficult. However, the tongue was not characteristic, there was little or no tympany, no spots or sudamina, and no hebetude or epistaxis, and the abdominal tenderness upon pressure, which was very marked, was in the left and not in the right iliac fossa. On the other hand, the conditions were in many indescribable ways like typhoid fever, and there was diarrhœa with continued fever lasting for more than two weeks and no discoverable cause for the symptoms. At the end of about two weeks and a half I was shown a brownish watery passage which had at the bottom a little gritty material almost like red gravel. This at once led to a suspicion that there must be some obstruction, and it was determined to give small doses of aperient medicine, but before this was carried into effect the patient passed two masses which were almost circular and about an inch and a half or an inch and three-quarters in diameter, and from this time there was no more fever. A few doses of a mild aperient brought away several more fecal masses and the tenderness in the left iliac fossa was relieved; the patient speedily recovered her appetite, and convalesced without further drawback.

Case V.—When I took charge of the wards of the Pennsylvania Hospital on August 1, 1879, I found there a woman fifty-eight years of age suffering with jaundice, great enlargement of the liver and of the abdomen generally, and constant fever. There were exacerbations of the fever about once a week, during which the temperature would run up to 103° F., and these attacks were always ushered in by a violent chill. The symptoms had been considered to be due to cancer of the liver or possibly abscess, and as the patient had already been more than two months in the hospital it had been pretty positively prognosticated that her span of life was rapidly drawing to its close. After having charge of the case for a week or more it struck me that the abdomen was very hard and doughy, and that it

could certainly not be amiss to give some purgative medicine. Accordingly she was ordered a pill of ext. belladonnæ gr. $\frac{1}{12}$, ext. nucis vomicæ gr. $\frac{1}{4}$, ext. colocynth. comp. gr. ij, one to be taken three times a day. After taking this pill for eight days there was a note made that "she has been having from three to six stools daily and has passed a vast amount of very consistent old clay-colored feces." Inquiry into the history of this patient elicited the fact that for years she had been habitually constipated, that frequently she would have no movement of the bowels for a week at a time, and sometimes as much as three weeks had been allowed to pass without any. Gradually, under the treatment, the fever ceased, the attacks of chill followed by increase of fever did not recur, the jaundice disappeared, the liver grew smaller and the stools became of natural color, and at the end of three months the woman was discharged cured.

Case VI.—Above five or six years ago I saw a woman of perhaps sixty years of age who had a small tumor which projected from beneath the arch of the ribs on the right side. After a careful consideration of the symptoms and physical signs, I concluded that it was an enlarged gall-bladder containing gall-stones. This patient had long been subject to attacks of jaundice which would be accompanied by pain in the region of the tumor. Before and during each attack she was constipated, and she said she was always well so long as her bowels were freely opened, but that so soon as they began to be at all confined she feared and anticipated an attack of the pain and jaundice. I prescribed for her such a pill as has already been so often mentioned, and advised her never to allow herself to be constipated, but to take the pills sufficiently often to produce one movement of the bowels every day or at least every second day. Afterward she fell into the hands of another physician, who ascribed the tumor to some other cause and told her on no account ever to take any aperient medicine. This advice she followed, and not long afterward she had one of her attacks, of which she died. An autopsy showed an enlarged gall-bladder containing calculi.

Case VII.—A woman of about sixty-five is affected almost exactly as was the one just described. She has a tumor projecting from

beneath the arch of the ribs on the right side and for the past four years has been subject to attacks of jaundice with pain in the region of the tumor, and some disturbance of the stomach, manifesting itself by vomiting. During and before the attacks she is constipated. This patient has been advised that constipation is her worst enemy, and as she has learned to appreciate the fact herself, she never allows it to continue long. In the past two years her attacks have been very much less frequent and less severe than in the two previous years. The most careful search has failed ever to lead to the discovery of any gall-stones in her movements.

I know of two or three women, all of them advanced in life, who for more than ten years past have seldom allowed a week to pass without taking a greater or less number of the previously mentioned pills and much more frequently they take one every night or second night. This particular combination does not seem to lose its effect after it has been taken for a long time, but, on the contrary, usually a less number of the pills will suffice after the patient has used them for some time than was the case when the medicine was commenced.

There is one form of constipation which I have never been able to treat to my satisfaction, and it is that which is especially apt to occur during convalescence from typhoid fever, but sometimes also after attacks of other acute diseases. It is a very common occurrence in my experience after an attack of typhoid fever and when the temperature has fallen to subnormal, as always happens, that where the bowels are moved, not having been so for four or five days, there is an attack of pain which is very severe, with utter inability on the part of the patient to rid himself of the fecal mass. This contingency, which arises in perhaps one-quarter of the total number of cases, cannot be explained upon the

ground that the amount of fecal matter which descends from the colon is so much greater than the natural forces discharge on other occasions without difficulty or even pain. It seems as if the matter was of such a consistence, or descends in such a form, as to fill the rectum so completely that the unaided efforts of nature fail entirely for a long time to discharge it. Patients, when such attacks come upon them, are weakened by the disease from which they are just recovering and their nervous systems are so exhausted that they are ill able to contend with any further adversity; consequently, men who in health are strong and bold will lie in abject fright with the sweat rolling off them, and ready almost to cry with the pain and nameless state of helplessness in which they find themselves, because they feel the most urgent need to have the bowels opened and are utterly unable to accomplish it. The very attitude of people in this condition is characteristic and their efforts to relieve themselves most violent, until finally, paralyzed with pain and terror, they give up, and lie helpless and exhausted until something is done to relieve them. The position in bed assumed under these circumstances is one that, once seen and fully understood, will never be forgotten: the patient usually lies somewhat turned upon one side so that the weight of the lower part of the body rests upon one buttock with the other raised from the bed, then the shoulders are elevated and their weight supported upon the arm on the same side. No doubt, if a person was left in this condition, nature would, in time, relieve the difficulty, but the remedy for the physician to make use of is a very simple one, and efficacious if properly applied: it is the injection of warm water. The physician should not content himself

by simply giving directions to a nurse, but should himself superintend the administration of the injections, for frequently several will have to be given before the desired effect is produced. The proper method to be pursued is to prepare a quart of warm water, and if the administration of this does not give complete relief, in five minutes a second should be administered, and a third, and so on until relief is obtained. Sometimes as many as four or five will be required before this is accomplished.

To prevent the occurrence of this accident is a matter which has often engaged my thoughts, but as yet I have failed to get any light upon it. There are no symptoms that I have learned to distinguish to enable me to predict when the accident is likely to occur in either typhoid fever or other diseases, however much I may have been on the lookout for it, and I am afraid to give aperient medicine in typhoid fever, which would seem to be the only possible preventive, lest it should have an injurious effect upon the intestine, which presents the prime lesion in this disease.

The cases related have all been of exceeding interest to me and very instructive. They would seem to offer as much room for the exhibition of special skill in understanding and coping with new and unforeseen combinations of symptoms in disease as any can. There are no rules which can be laid down with regard to symptoms to be looked for, as there are in all the diseases we are ordinarily called upon to contend with; therefore, the personal equation enters more largely into their successful management than usual, and a physician has rare opportunities to display his acumen as a diagnostician.

The lessons I have learned from them have been various, and I will endeavor to describe them in detail for the benefit of those who may meet with parallel cases. The first case described shows how insidiously fecal accumulation may take place and how it may induce quite serious symptoms which have no very evident connection with their cause. The attacks of chill followed by fever and accompanied by nausea and vomiting continued for at least four or five years before their true cause was even suspected, and light was obtained as to the cause in a manner almost accidental, by the discovery that the rectum was full of sticky, clayey, fecal matter. It was difficult to believe there could be fecal accumulation in this case, for the history was clear that previous to most of the attacks there had been no apparent constipation, the bowels being moved every day, and before the discovery was finally made by the digital examination there had been diarrhœa. Some of these movements I myself saw, and they were large and loose. Nothing induced me finally to make the examination but a careful attention to the fretful description, given by the patient, of her own sensations, and this in opposition to the equally determined conclusion of the nurse that the complaints of irritating sensations and a feeling that the rectum was still full, were all nonsense. I have learned to think that as physicians we cannot too closely give heed to the descriptions by our patients of their own sensations, hearing patiently and without interruption all they may have to say that is at all pertinent to the subject, and then, of course, depending equally entirely upon our own experience and knowledge to draw the proper inferences. I made my diagnosis then of fecal obstruction

or accumulation in this case in despite of the fact that the patient had had her bowels opened every day for two months past; that for several days she had had diarrhœa, having several loose movements each day, and in despite of the fact that her nurse was convinced that the bowels had been too much and not too little opened, and I made it simply by listening to, and giving due weight to, the complaining descriptions of the patient of her sensations, and then insisting upon making a digital examination of the rectum.

Case II. teaches, from a slightly different point of view, the same lesson that is to be learned from the first, that the mere having the bowels opened by no means proves that there cannot be fecal accumulation, and that in sufficient amount to occasion grave disturbance of health. This woman, when recovering from an acute disease, was seized with constipation, and nausea, and vomiting. The constipation was apparently easily relieved by the use of as simple and mild a remedy as a seidlitz powder, for the administration of one always induced one or two watery passages, but still the other two symptoms persisted. It was only the discovery of the doughy induration along the line of the colon in an otherwise exceedingly flat abdomen that lead to the correct diagnosis, and this was fallen upon almost by accident, or perhaps instinct, in the course of a search for some explanation of the exceedingly puzzling and alarming condition. The cure was effected only after giving for a whole week the aperient pills, and I was able to persist so long, confidently predicting the result because my previous experience enabled me to be entirely satisfied in my own mind what the cause was, when once the idea had struck me.

The symptoms in Case III. resembled more nearly those presented when there is a foreign body in the rectum than anything else, and the digital examination revealed that the pouch was empty except for the one small mass which was like a calculus, and which the patient's unaided efforts, injections, and pills entirely failed to remove.

The diagnosis was so obscure that it was only arrived at after between two and three weeks in Case IV., which was certainly one of surpassing interest. The symptoms so nearly resembled those presented in typhoid fever as to be very misleading, and it was only by a careful analysis of them that the correct conclusion was reached, that it was not a case of this disease, and after reaching this conclusion we were obliged to wait for ten days before the true solution of the difficulty came to light. It is instructive and well worth remembering that obstruction of the sigmoid flexure alone, and this without doubt existed in this case, could cause so serious and complicated a train of symptoms. One of the most marked symptoms was the very decided tenderness upon pressure in the left iliac fossa over the region of the sigmoid flexure while there was none in the right. The diarrhœa, which was quite severe from the beginning, was induced by the doses of sulphur taken, and this, in fact, seemed to be the starting-point of the illness and was its direct exciting cause.

In Case V. the patient was thought to have cancer or abscess of the liver, but a suspicion of the true state of affairs was easily reached, for the abdomen had to a marked degree the peculiar doughy induration that is so characteristic to those who have learned to recognize it. To feel sure of the existence of fecal

accumulation after once my suspicions were guided in the right direction was easy, and to prove this view correct, was equally so, for, after taking the purgative pills for eight days there was ocular demonstration of its existence. The knowledge, however, that this alone was the cause of all the very serious symptoms present, could only be reached by a process of exclusion and in the course of months, as the various disturbed organs and functions gradually returned to their natural condition when the irritating cause was removed, and the organic changes, one after another, disappeared with the relief of the fecal accumulation. It is a rare occurrence for fecal accumulation to continue until such dangerous conditions are induced as happened in this instance, and if relief had not been obtained when it was, or certainly but little later, the case must soon have had a fatal termination.

Cases VI. and VII. are interesting as showing how materially the comfort and well-being of persons suffering with gall-stones may be affected by the condition of the bowels. Careful attention to this matter seemed very necessary in both of them, and their comfort was dependent upon their taking aperient medicine in such quantities as to produce regular movements of the bowels. It is likely that over-purgation in such cases would be as injurious as the contrary condition was proved to be.

The cases related, when considered collectively, teach many and valuable lessons. In the first place, they show the surpassing wisdom of the advice, which is commonly attributed to Bright, never to pronounce an opinion in a case of abdominal tumor without first

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having purged the patient, and they show further that the manner of the purging is more important than its mere performance. One or two doses of any of the ordinary purgative medicines cannot be depended upon to accomplish the purpose; the doses must be small and frequently repeated, and the course must be persisted in for some time. The pills of belladonna, nux vomica, and compound extract of colocynth are, in my experience, unfailing, if their use is continued long enough. Large, single doses of medicine may produce watery movements as under ordinary circumstances, but they seem to fail entirely to dislodge the offending fecal matter which must lie in the sacculi and at the sides of the large intestine, as it is so often seen at post-mortem examinations. The large doses seem to throw the intestine into a short-lived spasm of violent activity, the result of which is the secretion of a large amount of watery matter which comes away leaving the condition of affairs almost totally unchanged. The medicine must be given in such small and slowly acting doses as to give time for the peristaltic movement of the whole intestine to be increased, and the result will be certain. The amount of fecal matter that can accumulate in the intestine without serious disturbance of health so long as a central channel is open is amazing, but a time must come when a sufficient quantity of this will move from its resting places in the sacculi and at the sides of the canal to cause a more or less complete block of the whole calibre of the bowel, and then nature can never rest until the way is clear again. If by some means, either by the unaided efforts of nature, or with the help of a purge, the channel is not soon

opened, nausea and vomiting are inevitable. For the production of these two latter symptoms the block must be complete, and the truth of this statement is evidenced by what occurred in the first five cases narrated. In Cases I. and II. the block was complete, and there were nausea and vomiting. In Case IV. the block was probably very nearly absolute, for nothing passed but thin watery passages, and in that case there was nausea, but no vomiting. In Cases III. and V. there was every reason to believe that at no time was the obstruction complete, and in neither of these cases was there marked nausea or vomiting. It is strange that all these patients should have been women, and for this no adequate explanation is at hand which satisfies my mind, for I consider the old explanation that women are careless about having their bowels moved a very poor and insufficient one. The two combinations of medicine which have been recommended in pill form proved efficacious in every case except the third, and in that instance it subsequently appeared that the mass to be passed was too large for the anal orifice, which was contracted and the muscle hypertrophied as a consequence of long-standing sphincterismus. So far as diagnosis is concerned, it is difficult to discuss, for the solution in each case differed so materially from that in every other one that no rules can be laid down. The physician must depend largely upon his individual acuteness in diagnosis, not allowing himself to be led astray either by a distinct history that in the past weeks or months his patient has had the bowels moved sufficiently often, or by the fact that single doses of purgative medicine produce watery movements of the

bowels in about the usual way. The diagnosis must be made from a careful inquiry into the history, which will often show tendencies that may lead to the correct solution, and this must be followed by an equally careful physical exploration of the abdomen, and if there is the slightest reason to suspect any intestinal difficulty, digital examination of the rectum.

[After the reading of the preceding paper :—]

Dr. J. M. DaCosta said : A point of particular interest is the occurrence of fever in these cases. I saw the fourth case with Dr. Meigs, and the fact that it simulated typhoid fever so closely is a matter of moment. I have seen a similar case in which there were almost identical symptoms, with an almost identical termination. We perceive from this case and the others referred to, that constipation may cause fever which is continued and may present the symptoms of a low type.

There is another point connected with the occurrence of constipation in fever to which Dr. Meigs did not have occasion to allude ; that is to say, sometimes after low fevers in which the state of constipation to which Dr. Meigs has called attention occurs, return of fever will be developed by the constipation. We grope around in darkness wondering what may be the cause, thinking that it is a true typhoid fever relapse, when by giving small doses of oil or of other laxatives, both the fever will disappear and the bowels be freely moved. I have seen the febrile action keep up for five or six days ; and I believe that a number of cases of so-called relapse in typhoid fever have their origin in the condition to which I call the attention of the College.

I will go further, I have reason to think that in some of these cases there may be other typhoid fever symptoms, such as headache, nocturnal delirium, abdominal tenderness, which will disappear when the bowels are freely moved. I have seen the same thing after remittent fever. It seems to me that the occurrence of constipation after fever, typhoid or malarial, may lead to the redevelopment of the febrile state, which may be considered a relapse, when in reality it is only the same kind of irritation or catarrhal inflammation of the bowels which in Case IV., of Dr. Meigs, produced a fever of low type, resembling typhoid fever.

Dr. James H. Hutchinson : Case IV. was probably not a case of simple constipation, but one of enteritis associated with the constipation. The enteritis, and not the constipation, was therefore the cause of the fever. Although I am, of course, aware that relapse is said to occur frequently in cases of typhoid fever in which there has been constipation during the original attack, I cannot at present recall a case in which relapse could be with any probability attributed to this cause. I have not myself seen a great deal of constipation follow typhoid fever.

THREE CASES

OF

PRESSURE UPON THE RECURRENT LARYNGEAL NERVE FROM DIFFERENT CAUSES,

WITH FIXATION OF THE LEFT VOCAL BAND IN THE PHONATORY POSITION.[1]

By

J. SOLIS COHEN, M.D.

[Read May 5, 1886.]

The portal for the breath of life is the glottis. Let its two doors become closed, and life becomes extinct within at most two minutes; but let only one of them remain closed never so long, and the interference with respiration may be too slight even to attract attention. The closing of the glottis and its opening are effected by muscles of antagonistic function; yet the opposing innervations are transmitted through fibres of the same nerve trunk, the recurrent laryngeal. Whether this dual innervation proceeds along the same set of fibres from two centres or from one double-acting centre, or whether it proceeds from separate centres along separate sets of fibres traversing the same trunk, as is most

[1] The unusual occurrence of having three such cases under observation at the same time, suggested their presentation for examination and comparison. Death in one instance, and neglect in another, limited the exhibition to one, but the laryngoscopic image was essentially the same in each instance.

plausible, is a problem for which neither experimental nor pathological physiology has yet furnished the solution.

When, therefore, a vocal band, as in the cases herewith presented, stands at continuous rest in the position normally occupied during phonation only, the question arises whether that fixed position is due to tonic spasm or contracture of the muscles which close the glottis on that side, or whether it is the result of paralysis of the muscle which holds the glottis open. Until recently the phenomenon has been attributed solely to neurotic paralysis of the posterior crico-arytenoid muscle, whose function it is with the help of its mate to keep the vocal bands asunder posteriorly, to hold the rima glottidis open for the needs of ordinary respiration, and to draw these bands still further asunder so as to open the glottis to its widest extent for the needs of extraordinary inspirations. This view is supported by the evidence of degeneration of the posterior crico-arytenoid muscle found in almost if not every such case examined *post-mortem*, little or no similar atrophy being discovered in the tissues of the antagonistic muscles. The uniformity with which this atrophic metamorphosis is known to take place, and its attribution to neurotic paralysis of the muscle, have, with other cogent considerations, led an able, accurate, and most painstaking investigator, Dr. Felix Semon, of London, to the conclusion that organic disease of the roots or trunks of the motor nerves of the larynx is likely to become earliest and sometimes exclusively manifested in paralysis of the posterior crico-arytenoid muscle. This view is the one generally entertained and endorsed by laryngologists. An opinion diametrically opposite, on the

other hand, and one with which I am more inclined to accord, has been expressed by Dr. Hermann Krause, of Berlin, based on clinical observation, on neural analogy, and on physiological experiment. In its briefest expression this view maintains that, in analogy with neurotic manifestations in other portions of the body, organic irritation of the recurrent laryngeal nerve produces spasm of all the muscles supplied by its fibres, both those which preside over the patency of the chink of the glottis for respiratory purposes, and those which preside over the approximation of its edges for purposes of phonation, cough, and expulsion of foreign material; but that, inasmuch as the number and mass of muscle and nerve fibres preponderate in the domain for closure, the equilibrium maintained by the respiratory centre under normal conditions is overpowered, so that the spasm remains manifest in the closing phase only, despite the coexistant spasm of the weaker dilator muscle. The opinion held, then, is that the phenomenon of permanent fixation is due to the overpowering contracture or tonic spasm of constricting muscles of the larynx, and not to a paralysis of a dilating one. The atrophy of the dilating muscle is attributed to its mechanical immobility and not to its paralytic immobility. On the other hand, those who believe in primary paralysis of the dilator muscle consider this spasm to be a secondary contracture, and not a primary one. The theory of spasm rather than paralysis, seems to be supported by the first case I had intended to show this evening, but since this paper has been announced, I have learned that the patient died suddenly, away from home, under circumstances that deprived me of the

satisfaction of a projected examination *post-mortem.* Briefly the case is as follows.

Aneurism of the arch of the aorta and of the left subclavian artery with fixation—spastic immobility?—of the left vocal band in the phonatory position. Mr. X. Y. Z., æt forty-four, formerly a corporal of artillery, and since June, 1865, a machinist, applied November 15, 1885, at the Philadelphia Polyclinic on account of great dyspnœa said to have been but of six months duration, although shortness of breath had existed for fully five years, eventually accompanied with frequent cough, and expectoration of frothy white mucus. Coughing produced great pain within the left side of the thorax. Pain in the left side and in the back had commenced about ten years before, and was always reproduced by excitement, although there was no consciousness of cardiac palpitation. Pains in the epigastric and cardiac areas had existed almost continuously for five years. These were followed by pains radiating along the left arm and up the left side of the neck into the head, where they had remained continuous for the last six months. Dizziness in walking had been produced for some months, compelling interruptions for rest. Dizziness was nearly as bad on standing. About five years before, the voice began to undergo change. It was now weak and diphthonic, the higher pitched tone being metallically shrill and squeaky. Respiration was embarrassed spasmodically on exertion, but during rest in the sitting position was fairly quiet at a rate of from twenty to twenty-four per minute. The patient felt much better in the open air than in a room. The pulse was about 86 per minute at the right wrist and over a pulsatile, expansile tumor below the clavicle and second rib of the left side, but it was almost imperceptible and apparently irregularly intermittent at the left wrist. The pupils were equally dilated.

There was a history of inflammatory rheumatism in 1866, with subsequent histories of intermittent fever and of the primary lesion of syphilis. Appetite and digestion were reported good, with irregularly constipated habit. Emaciation was steadily progressive.

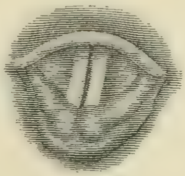
Physical exploration of the thorax revealed aneurism of the arch of the aorta and of the left subclavian artery. Laryngoscopic in-

spection revealed tense immobility of the left vocal band in the phonatory position—or rather, with its posterior extremity, a fraction of a line beyond the middle of the glottis, with slightly concave or semielliptic outline of the immovable vocal band (Fig. 1) representing the left half of the ellipse normally produced by the simultaneous action of the two sides in phonation (Fig. 2). The

FIG. 1.

In respiration.

FIG. 2.



In phonation.

aneurism of the aorta compressing the inferior laryngeal nerve as it winds around the arch of that vessel in its recurrent course to the larynx, explained the cause of the fixed position of the vocal band. This fixed position I attributed to preponderating spasm of the group of adductor muscles rather than to paralysis of the single abductor, because of the shrillness of the voice, and the presence of an overtense vocal band to account for that shrillness or increase of pitch; complete tension requiring contraction and not relaxation of the posterior crico-arytenoid muscle. The patient, who had passed out of my hands, died suddenly while walking in a garden on April 12th, but the manner of death is unknown, whether by spasm of the glottis or by rupture of the aneurism, probably by the latter. The physician summoned after death suspected disease of the heart.

A second patient with a similar permanent fixture of his left vocal band in the phonatory position, is now in the room adjoining. His phonic tone is not shrill as in the other case, but is apparently normal; as is usual in these instances. Consequently, the question as to contracture or paralysis can find no answer in his voice.

There is no aneurism detectable in the chest of this man; but that there may be aneurism pressing posteriorly on the recurrent nerve without physical evidences on auscultation, has been proved not only by a post-mortem examination in a case of my own[1] in which such a correct diagnosis had been made by exclusion in a man with a similar laryngeal defect; but by similar post-mortem discoveries by others. In this case, however, the fixture is not attributed to aneurism, but to another source of irritation, while the inference of contracture is based upon the occasional presence of tremors and even of spasm in the right vocal band, so that at moments the complete picture of the bilateral malady is manifest for a few seconds.

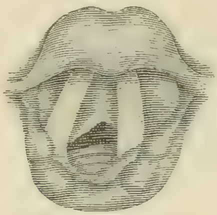
Some eight or more years ago I reported a case of permanent bilateral involvement of this kind preceded by the unilateral lesion. This patient died a few weeks ago, and the structures implicated, muscular and nervous, are undergoing study by Prof. Osler. It is probable that the case before us may become a similar instance of bilateral lesion following a unilateral one. The essential history of the case herewith presented, narrated as briefly as is consistent with its salient features, is as follows:

Fixation of left vocal band in the phonatory position, with tracheal constriction from enlarged thyroid body, and occasional spasm of the mobile vocal band of the right side. X. Y. Z., æt. forty-four, an old private of three years military service: of late years a longshoreman, accustomed to heavy work, lifting large bales of merchandise, applied to the Polyclinic with great dyspnœa and marked tracheal stridor, chiefly inspiratory: with a history of continuous difficulty of breath-

[1] Amer. Journ. Med. Sci., July, 1883, p. 89.

ing preventing sleep, and frequent suffocative attacks of spasm of the glottis especially at night. About one year before, while working actively in a place much exposed to draught, he began to get very hoarse so that he could barely whisper, but he had no fever and no embarrassment in respiration. Cough and expectoration ensued. In about a month his voice began to improve and became gradually restored, but cough and expectoration continued. About August last breathing became embarrassed, became associated with wheezing, and attended with a sense of weight at the epigastrium. This embarrassment in respiration steadily augmented. Just before applying at the Polyclinic, dysphagia became added to his symptoms, and he almost choked when taking food whether solids or liquids. The great stridor made laryngoscopic inspection the first step in investigation. The left vocal band was found fixed in the phonatory position. (Fig. 3.) Both bands were congested and thickened. The right

FIG. 3.



Fixation of left vocal band in phonatory position, with involution of tracheal wall of same side.

band and other laryngeal structures moved freely and normally in respiration and phonation, and on phonation even the supra-arytenoid cartilage of the affected side became bent inward and forward. The great stridor could not be accounted for by the encroachments upon the open glottis, for one-half was entirely normal in proportions and presented ample room for free respiration. Pressure on the trachea was, therefore, suspected, and further inspection revealed an involution of the tracheal wall on the left side and extending anteriorly. External examination of the neck revealed an indurated enlargement of the left side of the thyroid gland of the size of a walnut. The inference following was, that the left lobe of the thyroid body dipped

down so as to press both upon the trachea and upon the recurrent nerve; hence the immobile condition of the left vocal band, and the tracheal stridor. Tendon reflexes were normal. There was slight bilateral relaxation of the palate which did not respond to titillation Physical examination of the chest revealed small scattered bilateral areas of bronchial breathing most marked along the course of the primitive bronchi. These were attributed to the interruption of the normal tracheal sounds by the involution of the tube acting as a tumor or foreign body. Emphysema existed at the upper part of the left lung, most marked behind. The cardiac impulse was felt at the epigastrium. There was some insufficiency of the mitral valve. No evidence of aneurism could be elicited.

The family and personal history were negative. Nevertheless twenty grain doses of potassium iodide were given three times a day, with marked benefit from the very first; and within a few days dysphagia ceased, tracheal stridor ceased, dyspnœa disappeared except on exertion, and the thyroid gland underwent resolution until now its enlargement is hardly patent to inspection though distinctly perceptible to palpation, especially during movements of deglutition. Involution of the tracheal wall can still be made out on laryngoscopy; the left vocal band is still fixed in the middle line, but the mobility of the supra-arytenoid cartilage has apparently ceased. Within a fortnight or so I have noticed on several occasions that, under the excitement of instructions to phonate strongly or rapidly, or to make deep and rapid inspirations, the hitherto unimpaired vocal band of the right side is sometimes tremulously inobedient to the will, and that once in a while it occupies the middle line for a few seconds despite the strongest efforts at inspiration, which, in fact, increase the difficulty, and produce that characteristic stridor in inspiration which accompanies the bilateral condition under consideration. Certainly the temporary closure of the glottis is at present due to clonic spasm of the adductors of the hitherto sound side of the larynx. Whether this participation of the sound side is indicative of the encroachment of a central lesion which has hitherto been unilateral, or whether it is indicative of reflex motor disturbance from involvement of the sensory fibres of the pneumogastric of the side primarily affected, a local lesion first affecting the recurrent fibres only and

subsequently the other fibres of the pneumogastric, is a question for future study. The only abnormal physical thoracic signs at present are bronchial respiration in the upper part of the chest, with audible perception of the entire phase of expiration over the right bronchus posteriorly, prolonged expiration, as it is usually termed. This I am inclined to attribute simply to resonant deflection of the current against the involuted wall of the trachea. It is possible, however, that an aneurism may be pressing on the trachea, on the bronchi, on the recurrent, and even on the pneumogastric. This single lesion would account for all the phenomena, permanent and evanescent, except the dysphagia and the relaxation of the palate, and even for the relief under iodide of potassium. But I cannot detect such an aneurism. On the other hand, irritative lesion of the cortical centre, or of its communicating fibres with the medulla, or of the vago-accessorius nuclei would account for spasmodic contractions on one side, and the occasional tremors and momentary spasms of the other. The inward movement of the supra-arytenoid cartilage may be explained on the theory of additional innervation from the superior laryngeal, which, according to some authors, is said to innervate the arytenoideus muscle proper.

A third instance of fixation of the left vocal band, due to another cause, recently came under my notice accidentally, as indeed have several instances before.

Fixation of left vocal band in phonatory position, following left-sided pleuritic effusion with displacement of the heart. A young gentleman, nineteen years of age, was sent to me recently for inquiry as to the cause of a hoarseness of several weeks' duration. In addition to evidences of a passing laryngitis, which had produced the hoarseness, I found the left vocal band fixed in the middle line as in the other cases narrated. The hoarseness has passed away, voice is nearly normal, but the vocal band remains immobile in the median line. There is a history of left-sided pleurisy three years ago with effusion enough to push the heart to the right side. The region of the heart is rather prominent, there is slight dulness just below the

upper third of the sternum, the sound of the tricuspid valve is loudest just to the right of the sternum, and the other physical signs so far as I can detect them are of negative value. The theory at present entertained, for the case has not been thoroughly studied out, is that the displacement of the heart to the right and its probable hypertrophy have caused a strain upon the recurrent nerve by dragging the arch of the aorta forward, downward, and to the right, which has either developed a neuritis resulting in neuropathic contracture, or some adhesions of connective tissue or enlarged bronchial glands. Pressure by such swollen glands might induce such a neuritis. In either instance the physical condition of the larynx has nothing to do with the cause of the hoarseness on account of which the interior of the organ was originally inspected.

This case illustrates that when one vocal band is fixed in the middle line, the condition, however serious may be the lesion which has occasioned it, may remain undetected indefinitely. It does not interfere with respiration, for half a glottis is amply sufficient for that function. It does not impair the voice, for the immobile band is in the very best position for good phonation.

Given a vocal band fixed in the middle line in consequence of irritation upon the recurrent nerve, would this be due to spasmodic contracture of the entire group of muscles, or to simple over-action of the adductor portion of that group, because the abductor has become paralyzed?

That tremor precedes spasm is seen in the second case narrated; analogically to what has been witnessed in experiments upon the recurrent nerves, as performed by Krause and others, when fibrillary tremors precede the well-known traction of the vocal band to the middle line.

The great office of the laryngeal nerves is to secure space for respiration, their auxiliary office is to provide means for phonation and cough. While it seems, *a priori*, suitable that the importance of the dilators of the glottis should have caused them to be invested with special immunity, rather than with special vulnerability, it may be argued, on the other hand, that as the main office of the motor laryngeal nerves is to maintain the patency of the glottis for breathing purposes, serious injury to those nerves would compromise the integrity of that office. The reply to this argument would be, that an injury completely destructive of the conductivity of the nerve, leaves the vocal band immobile in a position which does not interfere with the respiratory function.

A hypothetical explanation for these apparent contradictions based upon general physiological principles is offered by my brother Dr. Solomon Solis Cohen. It adopts, in a modified form, the as yet undemonstrated theory of the existence of a cortical motor centre for the larynx, and assumes that the respiratory (abductor) fibres of the recurrent nerve offer less resistance to the passage of nerve-currents than do the phonatory (adductor) fibres; an assumption made highly probable by the ascertained laws of nerve-resistance. The direct energization of a muscle of a limb, it is well known, proceeds *via* an anterior nerve-root from the motor cells of the gray matter of the spinal cord—which may be compared to the local battery in a telegraph line—while the impulse which liberates this energy in obedience to the will, proceeds from a centre for voluntary motion in the cerebral

cortex—the line battery. It may also be set free by so-called reflex action, under the stimulus conveyed from the periphery by an afferent nerve; the peripheral impression becoming the line battery. Applying these facts and hypotheses to the larynx, the following conclusions are drawn. The local battery for laryngeal motion is the nucleus of the spinal accessory nerve in the fourth ventricle. From this proceed two sets of fibres, one, of low resistance, to the dilator muscles, another, of higher resistance, to the constrictor muscles. The ordinary dilatation of the glottis for respiration, which is accomplished by a partial contraction of the dilator muscles, is an automatic function of organic life, under the control of the respiratory centre in the medulla, and takes place with the liberation of the minimum amount of energy by the central motor cells. The respiratory impulses continuously traverse the recurrent nerve, and all other impulses become superimposed upon them. The effect of a first additional increment of nerve-force, which may be set free under psychic impulse from the cortex, is to stimulate further the dilator muscles, opening the glottis wider or to its full extent, as in forced inspiration. More intense psychic stimulation, awakening sufficient energy to overcome the resistance of the adductor fibres of the recurrent nerve, elicits in addition to the continuing action of the dilator muscles (which is necessary to complete the tension of the vocal bands posteriorly) the response of the constricting muscles, and the vocal bands are brought into position for phonation. Reflex stimulation of the centre by an afferent nerve, excites the coughing action of the constricting muscles.

If this explanation of the normal action of the vocal bands be correct, the positions they assume under pathological conditions become easily explicable.

An irritative lesion of the nerve tract, or of the centre, or such lesion of the cortex or of the (hypothetical) cortico-medullar tract as should remove psychic control from nuclear cells continuing to generate nervous force, would produce according to its degree, at one extreme, increased action of the dilating muscle (position of forced inspiration, extreme abduction), or at the other extreme, spasm of all the muscles, bringing the vocal bands into the phonatory position (complete adduction) and between these extremes would be the various manifestations of tremor, spasmodic cough, and even equilibrium (non-paralytic) in the cadaveric position.

In depressive lesions, when the nervous current is too weak to overcome the resistance of the adductor nerve fibres—as when the will fails in hysteria—or when there is partial interference with the generation or transmission of nerve-currents, as in some cases of disease or injury of the nerve tract, the constricting muscles would receive little or no influence, the dilating muscles would continue to act, and there would be presented the well-known picture of adductor paralysis, best exhibited in hysterical aphonia. In hysteria, perfect cough is still possible under reflex stimulation. In organic diseases presenting the same picture, cough should be impaired, but not absent, as organic disease of this nature is not likely to be bilateral.

Destructive lesions of tract or nucleus, if complete, cut off entirely the nervous stimulation ; all the muscles

fail to act; and the vocal bands are left in the cadaveric position (paralytic, complete relaxation).

This hypothesis, for the sake of simplicity, omits to take account of normal or pathological inhibition of respiratory acts, or of a possible complication offered by the unknown relations of the sympathetic system with the laryngeal function of respiration.

[After the reading of the preceding paper:—]

Dr. Harrison Allen said: The paper is such an evidence of close observation that it is impossible to do other than thank Dr. Cohen for his valuable clinical reports of these interesting cases. I have had but a limited experience in similar diseases, but such as it has been it leads me to confirm the statement of Dr. Cohen that the conditions are due to spasm rather than to paralysis. I am also inclined to think that a rheumatic condition underlies many of the symptoms. I had come to this conclusion from my own studies, and lately I have seen a paper by Dr. Knight, of Boston, in which he elaborates the statement and brings quite a mass of testimony in support of it. One of my cases, a man heavily built, brought up on a farm, and by occupation a carpenter, much in the open air, and who has a well-marked rheumatic state, is subject to attacks of spasm which have alarmed him and his family exceedingly. I am obliged to make local applications with extreme care for fear of inducing spasm. Spasm attending local applications in cases not associated with rheumatism or a previous rheumatic attack must be rare. Reference may be made in this connection to a case of a lady whom I have seen several times. There is fixation of one vocal band coincident with attacks of dyspnœa, and no matter what is done to change the condition, even so slight an application as the passage of a galvanic current from two or three cells, spasm ensues;—the face becomes blue and the condition is such as greatly to alarm the observer.

With respect to the theories for these conditions, all will agree that where there are a multiplicity of reasons given to explain any condition, the correct explanation remains unknown.

Dr. Edward T. Bruen: I have observed in three cases this tonic spasm of the vocal cord. One of the cases died from sudden spasm of the glottis, and at the post-mortem examination there was found an aneurism of the pulmonary artery, which had been previously suspected, and also marked dilatation of the aorta. The condition of the vocal cord was verified by one or two of my friends.

A patient with the same position of the vocal band, as described by Dr. Cohen, is now in the Philadelphia Hospital. For some reason, the larynx is considerably narrowed. The epiglottis hangs over the larynx, so that the chink is apparently much narrowed.

I had still a third case in which the same condition of the vocal cord existed, and in which the source of irritation was a morbid growth in the upper part of the œsophagus. This had penetrated the thyroid cartilage, but not the tissues within. There was a good deal of enlargement of the thyroid gland. This tonic spasm existed on one side, and I think that suddenly the same thing must have occurred on the other side, so as to produce fixation of both vocal cords, which naturally caused the death of the patient. The patient died suddenly while I was in the hospital. I had just left him, after having examined his larynx, which was about as usual, when a message was sent to me that the patient was choking; I hurried to him, but he was dead when I arrived.

Dr. Cohen has referred so fully to the literature of the subject that it is not necessary for me to go further. Dr. Semon, of London, alludes to the fact that in irritation of the recurrent laryngeal nerve, the abductor fibres of the muscle were possibly paralyzed sooner than the adductors; but as Dr. Allen has well said, we are not in a position to determine the exact cause of the condition.

It strikes me as a matter of interest that this condition of permanent spasm has been receiving so much attention of late, and I believe that it has a high value in the diagnosis of mediastinal disease, as, when present, it is quite an early symptom in these cases.

Dr. Charles E. Sajous: I have had occasion to meet with several cases of paralysis of the larynx. I have invariably observed the spasm alluded to. In regard to the theory of Dr. Semon that the abductor fibres become first implicated, I have seen two cases which seem to bear evidence in favor of the point advanced by him. One case struck me very vividly and I think it of importance to mention it in this connection. A man consulted me with bilateral paralysis. I found the vocal cords in almost complete adduction. Gradually, as time advanced, this paralysis in adduction became less and he had almost complete paralysis in the cadaveric position.

I had a second case under observation for some time at the Jefferson College Hospital in which the same phenomena were present. The case was first one of unilateral paralysis in adduction, finally passing into paralysis in the cadaveric position.

Dr. Cohen: The cases referred to by Dr. Allen in which there is a rheumatic condition, are entirely different from those I have reported

this evening. In the cases mentioned by Dr. Sajous, the paralysis was with the vocal bands at first in adduction, and then in the cadaveric position. The theory he refers to is that, the posterior crico-arytenoids being paralyzed, the preponderance of activity is manifested in the antagonists, the vocal bands thereby assuming the position in the median line of the larynx. When the paralysis becomes complete, involving both adductors and abductors, this preponderance is lost; the vocal bands recede from the median line of the larynx to the cadaveric position, in which they remain. It seems to me, however, that we can recognize three steps in the manifestation of the lesion—tremor, spasm, and paralysis. First, there is a tremor such as has been exhibited in the mobile vocal band of this individual; then a tonic spasm of all the laryngeal muscles, which brings the vocal bands into the phonatory position; and afterward, when the muscles become paralyzed, the picture becomes one of complete paralysis in the cadaveric position.

REMARKS ON PARASITES AND SCORPIONS.

By
JOSEPH LEIDY, M.D.

[Communicated May 5, 1886.]

I HAVE recently received for examination, from Dr. W. T. Belfield, of Chicago, three little nematoid worms, which, as stated in the letter accompanying the specimens were referred to him by Dr. R. W. Gelbach, of Mendota, Ill., who found them in the intestine of young anæmic, but otherwise, healthy cats. Both gentlemen believe them to be specimens of *ancylostomum duodenale*, and my examination has confirmed this opinion. On superficial inspection I supposed the worms might belong to *strongylus tubæformis*, a closely related parasite infesting the cat. The specimens, however, exhibit the same structure of the mouth as is described in the *A. duodenale* of man. Beneath the upper lip are four strong recurved hooks and within the lower lip a pair of hooks. The finding of this parasite in the cat in this country renders it probable that it may also infest man with us, and is probably one of the previously unrecognized causes of pernicious anæmia. The occurrence of the same parasite in the cat is also of interest, as heretofore it has only been noticed in man.

I take the opportunity to exhibit several excellent photographs of trichina in the flesh of the pig and in that of a young woman, sent to me by Mr. Eugene A. Rau, of Bethlehem, Pa. The photographs were accompanied by a letter giving an account of four cases of trichinosis which recently occurred in a family in that town. The pig used was raised at home and was stated at no time to have exhibited signs of being unwell. Two other hogs, raised on the same place, were examined and found not to be infested. Of the four persons infected, the mother, aged thirty-seven, and a daughter aged thirteen, died, while the father and a younger daughter were recovering. The photograph of the pork section exhibits many coiled worms encysted; those from the deltoid muscle of the girl exhibit numerous coiled and a few extended worms, lying loose among the muscular fibres.

Prof. Leidy also read a letter from Dr. V. Gonzalez, of Durango, Mexico, reporting the great prevalence of scorpions in the district, and the frequent fatality of their sting, especially among children, who die in a short time in convulsions. Dr. Gonzalez observes that a bounty is paid for the scorpions, and that some years over 100,000 are destroyed, but they still continue to be abundant.

[After the preceding verbal communication:—]

Dr. W. S. Forbes said: With reference to the second statement with regard to trichinosis, I would ask Dr. Leidy if the description which he gave of the trichina being found in pork, which is contained in the communication which he read before the Academy of Natural Sciences some forty years ago, was not the first description of the parasite being found in an article of diet? I ask this question because it has been repeatedly stated in Berlin that the trichina had been found there prior to that time.

Dr. Leidy: I believe that mine was the first notice of the parasite occurring in the pig. It had been previously discovered in man. I was led to find it in the pig after having seen it in man. Dr. Goddard noticed it in a subject in the dissecting room, in this city, several years before I observed it in the pig. The parasite was at first considered to be of no importance. Some years later in an epidemic of trichinosis in Germany, the parasite was discovered in many of the persons affected and in the meat that had been eaten. I think that it was Leuckart who made some experiments and referred to my notice of the trichina in the hog. The parasite was first discovered in man by Sir James Paget.

The President, Dr. S. Weir Mitchell: Is there anything known of the steps by which this worm referred to in the first communication finds entrance into the system of man, and of the way in which it is supposed to give rise to anæmia?

Dr. Leidy: It is supposed that the anchylostomum gains entrance to man through the drinking water, and if that is the case, the cats probably obtain it in the same way. If cats, in this country, obtain it from drinking water, it is probable that with us man may do so. It is curious that it should be found in the cat. Generally we find that similar parasitic worms are found only in animals closely related to one another. So far as we know, the ascaris vermicularis occurs in no other animal than races of men. The cat has its own ascaris and this is found in various species of cats all over the world. There is another found in the dog which is also found in the wolf. Again, the ordinary tapeworm of the dog is found in all sorts of dogs. I have a specimen from the wolf in the West and I have another which Dr. Kane obtained from an Esquimaux dog in the North. As I have said, worms of the same species in the same stage usually infest only animals which are closely related.

A CASE SIMULATING A MEDIASTINAL TUMOR WITH SPECIAL LARYNGEAL SYMPTOMS.

By

EDWARD T. BRUEN, M.D.,

PHYSICIAN TO THE PHILADELPHIA HOSPITAL AND ASSISTANT PROFESSOR OF PHYSICAL DIAGNOSIS, UNIVERSITY OF PENNSYLVANIA.

[Read June 2, 1886.]

There are certain lessons which can be drawn from the post-mortem examination of any case in which, during life, the diagnosis was subject to debate. I, therefore, desire to invite attention to a few considerations based upon certain specimens which I shall exhibit. The first represents a tumor originating in the œsophagus near its junction with the pharynx. The patient, A. C., was a well-nourished man, sixty-four years of age, six feet high, and weighing 186 pounds just before death.

He had been a shoemaker by trade and had been accustomed to labor with his shoemaker's last pressed against the sternum. His history was free from taint of rheumatism, syphilis, or Bright's disease. He entered the hospital in May, giving the history that in the preceding month he had commenced to suffer from paroxysmal dyspnœa which occurred without warning or assignable cause. He had few subjective symptoms,

and no pain; cough had been somewhat troublesome for a month before admission, with expectoration which was mucous, white, and frothy. Toward the close of his case, for he died August 20th, swallowing became difficult, although he was able to take liquid food to the last, and, therefore, did not lose flesh rapidly and presented the appearance of health.

The physical signs were in a measure those which usually accompany a mediastinal tumor, but disturbances of the respiration were most conspicuous. Whittier places the disorders of respiration in intrathoracic disease in the order of their value in diagnosis as follows: rapid breathing, dyspnœa on exertion, dyspnœa on change of position, dyspnœa explained by physical signs, orthopnœa, paroxysmal dyspnœa. In our patient there were paroxysmal attacks of dyspnœa which were somewhat increased on exertion but not on change of posture. These attacks could not be explained by the physical signs indicating pressure upon the branches of the bronchial tree, and the effects of pressure such as areas of collapse of pulmonary lobules, or areas of pulmonary consolidation, could not be detected. In a word, the dyspnœa seemed to be distinctly connected with pneumogastric irritation.[1]

Toward the close of his life, for he died August 20, 1885, the dyspnœa was more constant, and dysphagia became a pronounced symptom. His death occurred suddenly and was apparently due to obstruction of the larynx, probably spasm of the glottis. The autopsy revealed a tumor of the right lateral aspect of the œsophagus at its junction with the pharynx. The

[1] Dyspnœa as a Symptom of Intrathoracic Pressure, Philadelphia Medical Times, October, 1879. Bruen.

growth had increased backward and to the right, forming an enlargement which extended toward the vertebræ and also forward along the side of the thyroid cartilage. The right wing of the thyroid had been destroyed, but the larynx had not been penetrated. The new growth measured in vertical diameter two and one-fourth inches, and transversely about the same, although quite a pocket had been formed in the tissues beside the thyroid cartilage. Owing to this forward projection of the above named pocket the epiglottic cartilage was tilted backward; the cartilage itself was very large, measuring one and three-fourths inches from base to tip. The œsophageal mucous membrane was softened and ulcerated. The growth was found, on microscopic examination, to be an epithelioma of the squamous variety. The thyroid body was enlarged, but there were no other important changes in any of the organs of the body.

The upper portion of the sternum was removed and examined, and it was found to be an unusually thick bone especially for about two inches below the notch. Behind this part of the bone lay a large accumulation of adipose material, in fact, almost entitling one to call it a lipoma. The mass was three inches long by two inches across.

The case is fruitful in suggestions, especially as during life it was considered to be a case of intrathoracic tumor originally situated in the anterior mediastinum, and extending backward. In the latter stages of the disease this view of its anterior location was somewhat shaken by the increasing prominence of symptoms of pressure upon the œsophagus. The evidence which was regarded as indicating the origin of the growth in the anterior space, was that such pressure

symptoms as were present seemed to be only exercised in parts in the mesial line, and recognizing that growths in this region may attain a large size and grow with great rapidity, the increase of pressure symptoms upon the œsophagus did not attract as much attention as in a hypothetical case they might assume.

In the accompanying specimen we have an illustration of a tumor occupying the anterior mediastinum and exercising pressure upon parts in the mesial line, including the œsophagus. In the autopsy made by the writer,[1] on removing the sternum and cartilages they were found to be adherent on the right side to a mass which occupied the anterior mediastinum. The growth was seven inches long, measuring from the sternal notch, and terminated in a somewhat diffused thickening of the visceral pleura, which covered the anterior margin of the upper and middle lobe of the right lung. The growth was two and one-half inches broad. It overlaid the aorta, pulmonary artery, and the vessels of the neck. The calibre of the trachea was slightly diminished. The glands of the neck were unaffected on either side. The posterior mediastinal glands were very slightly enlarged along the sides of the trachea and upper bronchi. Laterally, at the lower portion of the growth, the pulmonary pleura was thickened at the line of contact of the tumor, but the lungs were free from any traces of disease. The new formation was of fibrous consistence, of a gray-white color, and through its centre a softened tissue was found. Microscopic examination showed the growth to be composed of medium-sized lymphoid cells mixed with spindle-shaped cells, and embedded in a

[1] See System of Practical Medicine, Pepper: Diseases of the Mediastinum, p. 866.

homogeneous stroma, or a stroma which consisted of reticulated fibres and wavy fibrous tissue. Other portions of the body were normal.

In some cases of tumor in the anterior mediastinum pneumogastric irritation is conspicuous. In a case reported by West in a similar situation the left phrenic and left pneumogastric nerves passed through the mass, and on dissection were found much thickened as they ran through the tumor. The tenth nerve measured three times its normal diameter and was pushed out of its course nearly an inch from the carotid. The recurrent laryngeal nerve was also thickened; the right pneumogastric and phrenic nerves were not involved.

Dysphagia is usually not observed in cases of growths in the anterior mediastinum, or is slight in proportion to other signs of pressure, but it may be simply a symptom of irritation of the intra-thoracic nerves or due to enlargement of the glands in the posterior mediastinum.

In the case of A. C. there was marked dulness over the upper piece of the sternum, and the heart-sounds were remarkably distant and muffled. These symptoms were explained by the finding, on post-mortem, of a thick and arched sternal bone with a cushion of fat behind it.

The condition of the general health in advanced cases of intra-thoracic tumor might offer some basis for differential reasoning upon the nature of a mediastinal tumor previously recognized by other physical signs. For instance, it is regarded as significant of aneurism if the general health is fair with marked pressure symptoms. But it is well established that sarcomatous growths may coincide with an appearance of fair health, and this adds perplexity to the differential diagnosis between

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aneurism and morbid growth. The case of A. C. can be appealed to as an illustration of an epithelial cancer which caused partial obstruction of the œsophagus, and yet the subject was fairly nourished to the end. There were no evidences of pulmonary invasion, the respiration was whistling or stridulous when the stethoscope was placed over the trachea, but there was fair respiratory murmur over each apex; nor was there any evidence of pressure on the roots of the bronchi such as is sometimes seen in cases of growths in the anterior mediastinum when the growth increases in size. This fact should probably have been given more weight in the differential diagnosis, but the physical signs of intrathoracic disease seemed so decided that the symptoms of laryngeal dyspnœa were incorrectly interpreted.

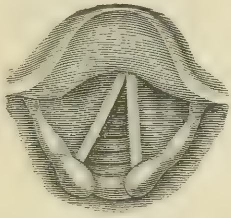
These laryngeal phenomena formed the most important features of the case. During life the overhanging epiglottis prevented a satisfactory view of the vocal cords, but when a glimpse was possible the right arytenoid was almost motionless and the vocal cord tensely stretched and in the phonative position in the median line. The voice was uniformly high-pitched, and when an attempt was made to give a higher note, was shrill. There was a good deal of venous congestion, but no glimpse of the growth could be made out; indeed, a prolonged laryngoscopic inspection could not be made, since the introduction of the mirror tended to bring on dyspnœa and laryngeal spasm. The dyspnœa and laryngeal spasm during life arose through implication of the pneumogastric nerve, for the right laryngeal must have passed through the growth.

The condition of the vocal cords in intra-thoracic disease is always worthy of special observation. I

have notes of two cases in which I noticed permanent or tonic spasm of the vocal cords.

C. Burke. æt. forty-nine. During life immobility of the left vocal cord was observed. The left arytenoid was almost motionless, and the cord was permanently fixed in the median line whether phonation was attempted or not. At times the left arytenoid was drawn sllghtly toward its fellow of the opposite side. The right vocal cord was normal in its action. (Fig. 1.) Inspection showed some

FIG. 1.



Position of vocal cords in C. Burke. From drawing by Dr. C. J. Seltzer, Medical Registrar.

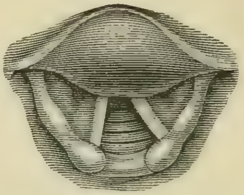
venous congestion of the laryngeal tissues. The voice, while ordinarily not loud, was abnormally high pitched, and when attempts were made to give a very high pitched tone, was shrill. The post-mortem confirmed the diagnosis established during life, viz., atheroma and dilatation of the aorta and also of the pulmonary artery. The specimens from this case I present at this time, but will refrain from entering into a detailed discussion.

Martin S., æt. fifty, a patient now living in the Philadelphia Hospital, is believed to have an aneurism of the ascending aortic arch near the transverse portion. Laryngoscopic examination is difficult on account of irritability of the throat. He complains of marked dyspnœa and possesses a high-pitched voice, which becomes louder, more shrill, and stridulous on making a high note. The epiglottis is large, pale, and pendant. Arytenoid of right side fixed in phonative position with cord—of course, in the median line as in the former case, while the left arytenoid and cord are normal in action.

(Fig. 2.) The mucous membrane appears thickened and shows venous congestion.

The explanation of this condition of the cords is still unsettled, being variously assigned to irritation of the nerve fibres of the recurrent which supply the adductors

Fig. 2.



Position of vocal cords in Martin S. From drawing by Dr. C. J. Seltzer, Medical Registrar.

of the larynx fibres, or by others to paralysis of the abductors, which are said to lie on the peripheral aspect of the nerve.

In connection with this condition of the vocal cords, it may be well to recall the original observations of Semon[1] "on the proclivity of the abductor fibres of the recurrent laryngeal nerve to become affected sooner than the adductor fibres, or even exclusively in cases of undoubted central or peripheral injury or disease of the roots or trunks of the pneumogastric, spinal accessory, or recurrent nerves." According to this observation, the vocal bands should remain fixed in the phonative position and recede to the cadaveric position only when the adductor filaments become paralyzed. He continues, this initiatory phase of the paralysis being usually unilateral does not markedly affect the voice and hence escapes recognition, laryngoscopic examination being rarely instituted until impairment of voice

[1] Arch. Laryn., July, 1881, p. 197. International Ency., Ashhurst, vol. 5.

has resulted from complete paralysis, with the cords fixed in the cadaveric position.

In the *Polyclinic* for January, 1886, a case of similar character to those just related is reported from the practice of Dr. J. S. Cohen by Dr. A. W. Watson. The intra-thoracic lesion in this case was aneurism of the aorta and left subclavian artery. The left vocal cord is described as fixed in the median line in the phonative position. The voice was stridulous and high-pitched. It would appear, therefore, that the position of the vocal cords might afford most valuable auxiliary data in the early diagnosis of intra-thoracic disease, and possibly from the relations of the recurrent laryngeal to the aorta and subclavian arteries the symptom might be looked upon as ranging itself particularly among those indicative of aneurism. But at the same time, to illustrate the multifarious meaning of any symptom connected with mediastinal disease, we must note the occurrence of this symptom in connection with cancer of the upper portion of the œsophagus, and the misinterpretation of the laryngeal phenomena.

In review, we may say that the case presented illustrates anew the complexity of the diagnosis of mediastinal disease. The liability to error in deciding the location of the process is well explained by the postmortem specimens. Indeed, the tumor was really situated in the neck, but as the patient had a short neck, very thick and fat, no swelling could be distinguished. The thick sternum with its underlying cushion of fat was misleading. Lastly the mode of death was unusual, since in disease of the œsophagus the termination is generally brought about by inanition, marasmus, hemorrhage, or perforation into adjoining organs. It

has also seemed possible that since the right wing of the thyroid cartilage was perforated, the laryngeal lumen must have been sensibly diminished and lodgement of food in the pocket formed in the œsophagus might have produced occlusion and brought on reflex spasm, or the lumen of the glottis already narrowed may have been occluded by the sucking into it of the folds of loose tissue. During the clinical study of the case some etiological importance was assigned to the question of avocation on the basis that sarcomata may claim a connection with direct irritation; but this, of course, was of minor importance especially as the only reason for forecasting that the supposed mediastinal tumor was a sarcoma, was based on the observation that tumors of the anterior mediastinum are usually of this variety.

[After the reading of the preceding paper :]

Dr. JAMES C. WILSON: Dr. Bruen has spoken of my having seen the case. I can only say that I quite agreed with Dr. Bruen and my colleagues in the hospital as to the obscurity of the diagnosis. One of the questions brought up at the consultation was with reference to the possibility of the symptoms being due to substernal abscess, abscess of the anterior mediastinum. This view of the case, I think, most of my colleagues were unable to accept, although some of the phenomena pointed strongly to it. This point was raised with the view of determining whether or not any surgical procedure should be instituted. After several careful examinations the diagnosis of abscess was considered inadmissible.

With reference to sudden death in cases of malignant disease of the œsophagus, I may state that within a brief period after this case was under observation, I had a case in the hospital where sudden death occurred in a negro woman aged sixty. At the post-mortem there was found quite a small malignant growth of the œsophagus, occupying about the same position as the tumor in Dr. Bruen's case. In my case there were no physical signs and only two symptoms, namely, occasional slight difficulty in swallowing solids and paroxysmal dyspnœa.

Dr. BRUEN: The question of abscess was specially developed by Dr. Agnew. There was a certain amount of œdema, redness, and tenderness above the sternum. The introduction of a drill through the sternum as a diagnostic measure was discussed, but was finally decided that it would not be prudent to do so. During the last week the symptoms of pressure on the œsophagus increased very rapidly. This, I think, was due to the accumulation of food or broken-down material in the pocket which I have demonstrated, and thus narrowing the œsophageal lumen.

Dr. JAMES H. HUTCHINSON: Dr. Longstreth has reminded me of a case of cancer of the œsophagus in my ward of the Pennsylvania Hospital. There was complete occlusion of the œsophagus and the patient had been unable to swallow for several days. I had given the patient up, but at the suggestion of my resident, Dr. T. S. K. Morton, I administered half a grain of cocaine. This enabled a quart of milk to be taken within half an hour. Subsequently the administration of one-twelfth of a grain enabled him to take a pint of milk. In this way his life was prolonged at least ten days.

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